This is a digital copy of a book that was preserved for generations on library shelves before it was carefully scanned by Google as part of a project to make the world's books discoverable online.

It has survived long enough for the copyright to expire and the book to enter the public domain. A public domain book is one that was never subject to copyright or whose legal copyright term has expired. Whether a book is in the public domain may vary country to country. Public domain books are our gateways to the past, representing a wealth of history, culture and knowledge that's often difficult to discover.

Marks, notations and other marginalia present in the original volume will appear in this file - a reminder of this book's long journey from the publisher to a library and finally to you.

Usage guidelines

Google is proud to partner with libraries to digitize public domain materials and make them widely accessible. Public domain books belong to the public and we are merely their custodians. Nevertheless, this work is expensive, so in order to keep providing this resource, we have taken steps to prevent abuse by commercial parties, including placing technical restrictions on automated querying.

We also ask that you:

+ *Make non-commercial use of the files* We designed Google Book Search for use by individuals, and we request that you use these files for personal, non-commercial purposes.

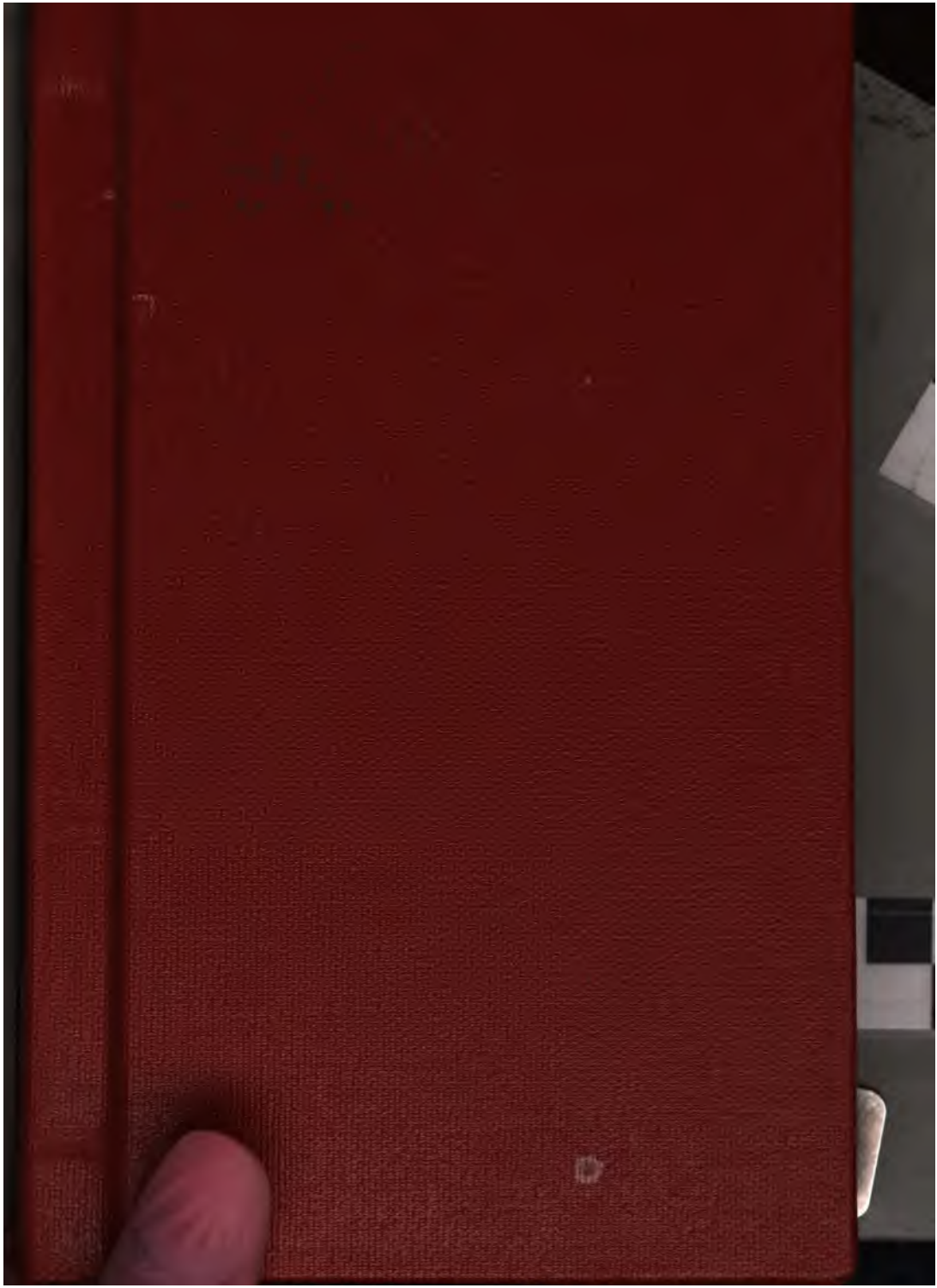
+ *Refrain from automated querying* Do not send automated queries of any sort to Google's system: If you are conducting research on machine translation, optical character recognition or other areas where access to a large amount of text is helpful, please contact us. We encourage the use of public domain materials for these purposes and may be able to help.

+ *Maintain attribution* The Google "watermark" you see on each file is essential for informing people about this project and helping them find additional materials through Google Book Search. Please do not remove it.

+ *Keep it legal* Whatever your use, remember that you are responsible for ensuring that what you are doing is legal. Do not assume that just because we believe a book is in the public domain for users in the United States, that the work is also in the public domain for users in other countries. Whether a book is still in copyright varies from country to country, and we can't offer guidance on whether any specific use of any specific book is allowed. Please do not assume that a book's appearance in Google Book Search means it can be used in any manner anywhere in the world. Copyright infringement liability can be quite severe.

About Google Book Search

Google's mission is to organize the world's information and to make it universally accessible and useful. Google Book Search helps readers discover the world's books while helping authors and publishers reach new audiences. You can search through the full text of this book on the web at `http://books.google.com/`



POPULAR LECTURES

ON

SCIENTIFIC SUBJECTS.

POPULAR LECTURES

ON

SCIENTIFIC SUBJECTS.

POPULAR LECTURES

ON

SCIENTIFIC SUBJECTS.

POPULAR LECTURES

ON

SCIENTIFIC SUBJECTS.

BY

H. HELMHOLTZ,

PROFESSOR OF PHYSICS IN THE UNIVERSITY OF BERLIN.

TRANSLATED BY

E. ATKINSON, PH.D. F.C.S.

PROFESSOR OF EXPERIMENTAL SCIENCE, STAFF COLLEGE.

WITH AN INTRODUCTION

BY

PROFESSOR TYNDALL.

NEW YORK:
D. APPLETON AND COMPANY,
72 FIFTH AVENUE.
1897.

2 19SU XL 1624
1/00 31150-192

504
H479a
v.1

LELAND STANFORD JUNIOR UNIVERSITY. LIBRARY

A.25453.
[illegible] 8 1898
[illegible]

AUTHOR'S PREFACE.

In compliance with many requests, I beg to offer to the public a series of popular Lectures which I have delivered on various occasions. They are designed for readers who, without being professionally occupied with the study of Natural Science, are yet interested in the scientific results of such studies. The difficulty, felt so strongly in printed scientific lectures, namely, that the reader cannot see the experiments, has in the present case been materially lessened by the numerous illustrations which the publishers have liberally furnished.

The first and second Lectures have already appeared in print; the first in a university programme which, however, was not published. The second appeared in the 'Kieler Monatsschrift' for May, 1853, but owing to the restricted circulation of that journal, became but little known; both have, accordingly, been reprinted. The third and fourth Lectures have not previously appeared.

These Lectures, called forth as they have been by incidental occasions, have not, of course, been composed in accordance with a rigidly uniform plan. Each of them has been kept perfectly independent of the others.

Hence some amount of repetition has been unavoidable, and the first four may perhaps seem somewhat confusedly thrown together. If I may claim that they have any leading thought, it would be that I have endeavoured to illustrate the essence and the import of Natural laws, and their relation to the mental activity of man. This seems to me the chief interest and the chief need in Lectures before a public whose education has been mainly literary.

I have but little to remark with reference to individual Lectures. The set of Lectures, which treats of the Theory of Vision, have been already published in the 'Preussische Jahrbücher,' and have acquired, therefore, more of the character of Review articles. As it was possible in this second reprint to render many points clearer by illustrations, I have introduced a number of woodcuts, and inserted in the text the necessary explanations. A few other small alterations have originated in my having availed myself of the results of new series of experiments.

The fifth Lecture, on the Interaction of Natural Forces, originally published sixteen years ago, could not be left entirely unaltered in this reprint. Yet the alterations have been as slight as possible, and have merely been such as have become necessary by new experimental facts, which partly confirm the statements originally made, and partly modify them.

The seventh Lecture, on the Conservation of Force, developes still further a portion of the fifth. Its main object is to elucidate the cardinal physical ideas of work, and of its unalterability. The applications and consequences of the law of the Conservation of Force are comparatively more easy to grasp. They have in recent

times been treated by several persons in a vivid and interesting manner, so that it seemed unnecessary to publish the corresponding part of the cycle of lectures which I delivered on this subject; the more so as some of the more important subjects to be discussed will, perhaps in the immediate future, be capable of more definite treatment than is at present possible.

On the other hand, I have invariably found that the fundamental ideas of this subject always appear difficult of comprehension not only to those who have not passed through the school of mathematical mechanics; but even to those who attack the subject with diligence and intelligence, and who possess a tolerable acquaintance with natural science. It is not to be denied that these ideas are abstractions of a quite peculiar kind. Even such a mind as that of Kant found difficulty in comprehending them; as is shown by his controversy with Leibnitz. Hence I thought it worth while to furnish in a popular form an explanation of these ideas, by referring them to many of the better known mechanical and physical examples; and therefore I have only for the present given the first Lecture of that series which is devoted to this object.

The last Lecture was the opening address for the 'Naturforscher-Versammlung,' in Innsbrück. It was not delivered from a complete manuscript, but from brief notes, and was not written out until a year after. The present form has, therefore, no claim to be considered an accurate reproduction of that address. I have added it to the present collection, for in it I have treated briefly what is more fully discussed in the other articles. Its title to the place which it occupies lies in the fact that it attempts to bring the views enunciated in the

preceding Lectures into a more complete and more comprehensive whole.

In conclusion, I hope that these Lectures may meet with that forbearance which lectures always require when they are not heard, but are read in print.

THE AUTHOR.

TRANSLATOR'S PREFACE.

In bringing this Translation of Helmholtz's Popular Scientific Lectures before the public, I have to thank Mr. A. J. Ellis for having placed at the disposal of the Publishers the translation of the third Lecture; and also Dr. Francis, the Editor of the 'Philosophical Magazine,' for giving me permission to use the translation of the fifth Lecture, which originally appeared in that Journal.

In addition to the Editorial charge of the book, my own task has been limited to the translation of two of the Lectures. I should have hesitated to undertake the work, had I not from the outset been able to rely upon the aid of several gentlemen whose names are appended to the Contents. One advantage gained from this division of labour is, that the publication of the work has been accelerated; but a far more important benefit has been secured to it, in the co-operation of translators who have brought to the execution of their task special knowledge of their respective subjects.

E. ATKINSON.

Staff College:
March 1873.

CONTENTS.

LECTURE PAGE

I. On the Relation of Natural Science to Science in General. Translated by H. W. Eve, Esq., M.A., F.C.S., Wellington College 1

II. On Goethe's Scientific Researches. Translated by H. W. Eve, Esq. 33

III. On the Physiological Causes of Harmony in Music. Translated by A. J. Ellis, Esq., M.A., F.R.S. . . . 61

IV. Ice and Glaciers. Translated by Dr. Atkinson, F.C.S., Professor of Experimental Science, Staff College . . . 107

V. On the Interaction of the Natural Forces. Translated by Professor Tyndall, LL.D., F.R.S. 153

VI. The Recent Progress of the Theory of Vision. Translated by Dr. Pye-Smith, B.A., F.R.C.P., Guy's Hospital:

i. The Eye as an Optical Instrument 197

ii. The Sensation of Sight 229

iii. The Perception of Sight 270

VII. On the Conservation of Force. Translated by Dr. Atkinson 317

VIII. On the Aim and Progress of Physical Science. Translated by Dr. W. Flight, F.C.S., British Museum 363

INTRODUCTION.

In the year 1850, when I was a student in the University of Marburg, it was my privilege to translate for the 'Philosophical Magazine' the celebrated memoirs of Clausius, then just published, on the Moving Force of Heat.

In 1851, through the liberal courtesy of the late Professor Magnus, I was enabled to pursue my scientific labours in his laboratory in Berlin. One evening during my residence there my friend Dr. Du Bois-Raymond put a pamphlet into my hands, remarking that it was 'the production of the first head in Europe since the death of Jacobi,' and that it ought to be translated into English. Soon after my return to England I translated the essay and published it in the 'Scientific Memoirs,' then brought out under the joint-editorship of Huxley, Henfrey, Francis, and myself.

This essay, which was communicated in 1847 to the Physical Society of Berlin, has become sufficiently famous since. It was entitled 'Die Erhaltung der Kraft,' and its author was Helmholtz, originally Military Physician in the Prussian service, afterwards Professor of Physiology in the Universities of Königsberg and Heidelberg, and now Professor of Physics in the University of Berlin.

Brought thus face to face with the great generalisation of the Conservation of Energy, I sought, to the best of my ability, to master it by independent thought in all its physical details. I could not forget my indebtedness to

Helmholtz and Clausius, or fail to see the probable influence of their writings on the science of the coming time. For many years, therefore, it was my habit to place every physical paper published by these eminent men within the reach of purely English readers.

The translation of the lecture on the 'Wechselwirkung der Naturkräfte,' printed in the following series, had this origin. It appears here with the latest emendations of the author introduced by Dr. Atkinson.

The evident aim of these Lectures is to give to those 'whose education has been mainly literary,' an intelligent interest in the researches of science. Even among such persons the reputation of Helmholtz is so great as to render it almost superfluous for me to say that the intellectual nutriment here offered is of the very first quality.

Soon after the publication of the 'Tonempfindungen' by Helmholtz, I endeavoured to interest the Messrs. Longman in the work, urging that the publication of a translation of it would be an honour to their house. They went carefully into the question of expense, took sage counsel regarding the probable sale, and came reluctantly to the conclusion that it would not be remunerative.[1] I then recommended the translation of these 'Populäre Vorträge,' and to this the eminent publishers immediately agreed.

Hence the present volume, brought out under the editorship of Dr. Atkinson of the Staff College, Sandhurst. The names of the translators are, I think, a guarantee that their work will be worthy of their original.

JOHN TYNDALL.

ROYAL INSTITUTION:
March 1873.

[1] Since the date of the foregoing letter from Professor Tyndall, Messrs. Longman & Co. have made arrangements for the translation of Helmholtz's *Tonempfindungen*, by Mr. Alexander J. Ellis, F.R.S., &c.

ON THE

RELATION OF NATURAL SCIENCE[1] TO GENERAL SCIENCE.

ACADEMICAL DISCOURSE DELIVERED AT HEIDELBERG, NOVEMBER 22, 1862,

BY DR. H. HELMHOLTZ, SOMETIME PRORECTOR.

TO-DAY we are met, according to annual custom, in grateful commemoration of an enlightened sovereign of this kingdom, Charles Frederick, who, in an age when the ancient fabric of European society seemed tottering to its fall, strove, with lofty purpose and untiring zeal, to promote the welfare of his subjects, and, above all, their moral and intellectual development. Rightly did he judge that by no means could he more effectually realise this beneficent intention than by the revival and the encouragement of this University. Speaking, as I do, on such an occasion, at once in the name and in the pre-

[1] The German word *Naturwissenschaft* has no exact equivalent in modern English, including, as it does, both the Physical and the Natural Sciences. Curiously enough, in the original charter of the Royal Society, the phrase *Natural Knowledge* covers the same ground, but is there used in opposition to supernatural knowledge. (Note in Buckle's *Civilisation*, vol. ii. p. 341.)—TR.

sence of the whole University, I have thought it well to try and take, as far as is permitted by the narrow standpoint of a single student, a general view of the connection of the several sciences, and of their study.

It may, indeed, be thought that, at the present day, those relations between the different sciences which have led us to combine them under the name *Universitas Litterarum*, have become looser than ever. We see scholars and scientific men absorbed in specialities of such vast extent, that the most universal genius cannot hope to master more than a small section of our present range of knowledge. For instance, the philologists of the last three centuries found ample occupation in the study of Greek and Latin; at best they added to it the knowledge of two or three European languages, acquired for practical purposes. But now comparative philology aims at nothing less than an acquaintance with all the languages of all branches of the human family, in order to deduce from them the laws by which language itself has been formed, and to this gigantic task it has already applied itself with superhuman industry. Even classical philology is no longer restricted to the study of those works which, by their artistic perfection and precision of thought, or because of the importance of their contents, have become models of prose and poetry to all ages. On the contrary, we have learnt that every lost fragment of an ancient author, every gloss of a pedantic grammarian, every allusion of a Byzantine court-poet, every broken tombstone found in the wilds of Hungary or Spain or Africa, may contribute a fresh fact, or fresh evidence, and thus serve to increase our knowledge of the past. And so another group of scholars are busy with the vast scheme of collecting and cataloguing, for the use of their successors, every available relic of classical antiquity.

Add to this, in history, the study of original documents, the critical examination of parchments and papers accumulated in the archives of states and of towns; the combination of details scattered up and down in memoirs, in correspondence, and in biographies; the deciphering of hieroglyphics and cuneiform inscriptions; in natural history the more and more comprehensive classification of minerals, plants, and animals, as well living as extinct; and there opens out before us an expanse of knowledge the contemplation of which may well bewilder us. In all these sciences the range of investigation widens as fast as the means of observation improve. The zoologists of past times were content to have described the teeth, the hair, the feet, and other external characteristics of an animal. The anatomist, on the other hand, confined himself to human anatomy, so far as he could make it out by the help of the knife, the saw, and the scalpel, with the occasional aid of injections of the vessels. Human anatomy then passed for an unusually extensive and difficult study. Now we are no longer satisfied with the comparatively rough science which bore the name of human anatomy, and which, though without reason, was thought to be almost exhausted. We have added to it comparative anatomy—that is, the anatomy of all animals —and microscopic anatomy, both of them sciences of infinitely wider range, which now absorb the interest of students.

The four elements of the ancients and of mediæval alchemy have been increased to sixty-four, the last four of which are due to a method invented in our own University, which promises still further discoveries.[1] But

[1] That is the method of spectrum analysis, due to Bunsen and Kirchoff, both of Heidelberg. The elements alluded to are cæsium rubidium, thallium, and iridium.

not merely is the number of the elements far greater, the methods of producing complicated combinations of them have been so vastly improved, that what is called organic chemistry, which embraces only compounds of carbon with oxygen, hydrogen, nitrogen, and a few other elements, has already taken rank as an independent science.

'As the stars of heaven for multitude' was in ancient times the natural expression for a number beyond our comprehension, Pliny even thinks it almost presumption ('rem etiam Deo improbam') on the part of Hipparchus to have undertaken to count the stars and to determine their relative positions. And yet none of the catalogues up to the seventeenth century, constructed without the aid of telescopes, give more than from 1,000 to 1,500 stars of magnitudes from the first to the fifth. At present several observatories are engaged in continuing these catalogues down to stars of the tenth magnitude. So that upwards of 200,000 fixed stars are to be catalogued and their places accurately determined. The immediate result of these observations has been the discovery of a great number of new planets; so that, instead of the six known in 1781, there are now seventy-five.[1]

The contemplation of this astounding activity in all branches of science may well make us stand aghast at the audacity of man, and exclaim with the Chorus in the 'Antigone': 'Who can survey the whole field of knowledge? Who can grasp the clues, and then thread the labyrinth?' One obvious consequence of this vast extension of the limits of science is, that every student is forced to choose a narrower and narrower field for his own studies, and can only keep up an imperfect acquaintance even with allied fields of research. It almost raises a smile to hear that in the seventeenth century Kepler was

[1] At the end of November 1864, the 82nd of the small planets, Alcmene, was discovered. There are now 109.

invited to Gratz as professor of mathematics and moral philosophy; and that at Leyden, in the beginning of the eighteenth, Boerhave occupied at the same time the chairs of botany, chemistry, and clinical medicine, and therefore practically that of pharmacy as well. At present we require at least four professors, or, in an university with its full complement of teachers, seven or eight, to represent all these branches of science. And the same is true of other faculties.

One of my strongest motives for discussing to-day the connection of the different sciences is that I am myself a student of natural philosophy; and that it has been made of late a reproach against natural philosophy that it has struck out a path of its own, and has separated itself more and more widely from the other sciences which are united by common philological and historical studies. This opposition has, in fact, been long apparent, and seems to me to have grown up mainly under the influence of the Hegelian philosophy, or, at any rate, to have been brought out into more distinct relief by that philosophy. Certainly, at the end of the last century, when the Kantian philosophy reigned supreme, such a schism had never been proclaimed; on the contrary, Kant's philosophy rested on exactly the same ground as the physical sciences, as is evident from his own scientific works, especially from his 'Cosmogony,' based upon Newton's Law of Gravitation, which afterwards, under the name of Laplace's Nebular Hypothesis, came to be universally recognised. The sole object of Kant's 'Critical Philosophy' was to test the sources and the authority of our knowledge, and to fix a definite scope and standard for the researches of philosophy, as compared with other sciences. According to his teaching, a principle discovered *a priori* by pure thought was a rule applicable to the method of pure thought, and nothing further; it

could contain no real, positive knowledge. The 'Philosophy of Identity'[1] was bolder. It started with the hypothesis that not only spiritual phenomena, but even the actual world—nature, that is, and man—were the result of an act of thought on the part of a creative mind, similar, it was supposed, in kind to the human mind. On this hypothesis it seemed competent for the human mind, even without the guidance of external experience, to think over again the thoughts of the Creator, and to rediscover them by its own inner activity. Such was the view with which the 'Philosophy of Identity' set to work to construct *a priori* the results of other sciences. The process might be more or less successful in matters of theology, law, politics, language, art, history, in short, in all sciences, the subject-matter of which really grows out of our moral nature, and which are therefore properly classed together under the name of moral sciences. The state, the church, art, and language, exist in order to satisfy certain moral needs of man. Accordingly, whatever obstacles nature, or chance, or the rivalry of other men may interpose, the efforts of the human mind to satisfy its needs, being systematically directed to one end, must eventually triumph over all such fortuitous hindrances. Under these circumstances, it would not be a downright impossibility for a philosopher, starting from an exact knowledge of the mind, to predict the general course of human development under the above-named conditions, especially if he has before his eyes a basis of observed facts, on which to build his abstractions. Moreover, Hegel was materially assisted, in his attempt to solve this problem, by the profound and philosophical views on historical and scientific subjects, with which the writings of his immediate predecessors, both poets and

[1] So called because it proclaimed the identity not only of subject and object, but of contradictories, such as existence and non-existence.—Tr.

philosophers, abound. He had, for the most part, only to collect and combine them, in order to produce a system calculated to impress people by a number of acute and original observations. He thus succeeded in gaining the enthusiastic approval of most of the educated men of his time, and in raising extravagantly sanguine hopes of solving the deepest enigma of human life; all the more sanguine doubtless, as the connection of his system was disguised under a strangely abstract phraseology, and was perhaps really understood by but few of his worshippers.

But even granting that Hegel was more or less successful in constructing, *a priori*, the leading results of the moral sciences, still it was no proof of the correctness of the hypothesis of Identity, with which he started. The facts of nature would have been the crucial test. That in the moral sciences traces of the activity of the human intellect and of the several stages of its development should present themselves, was a matter of course; but surely, if nature really reflected the result of the thought of a creative mind, the system ought, without difficulty, to find a place for her comparatively simple phenomena and processes. It was at this point that Hegel's philosophy, we venture to say, utterly broke down. His system of nature seemed, at least to natural philosophers, absolutely crazy. Of all the distinguished scientific men who were his contemporaries, not one was found to stand up for his ideas. Accordingly, Hegel himself, convinced of the importance of winning for his philosophy in the field of physical science that recognition which had been so freely accorded to it elsewhere, launched out, with unusual vehemence and acrimony, against the natural philosophers, and especially against Sir Isaac Newton, as the first and greatest representative of physical investigation. The philosophers accused the scientific men of narrowness; the scientific men retorted

that the philosophers were crazy. And so it came about that men of science began to lay some stress on the banishment of all philosophic influences from their work; while some of them, including men of the greatest acuteness, went so far as to condemn philosophy altogether, not merely as useless, but as mischievous dreaming. Thus, it must be confessed, not only were the illegitimate pretensions of the Hegelian system to subordinate to itself all other studies rejected, but no regard was paid to the rightful claims of philosophy, that is, the criticism of the sources of cognition, and the definition of the functions of the intellect.

In the moral sciences the course of things was different, though it ultimately led to almost the same result. In all branches of those studies, in theology, politics, jurisprudence, æsthetics, philology, there started up enthusiastic Hegelians, who tried to reform their several departments in accordance with the doctrines of their master, and, by the royal road of speculation, to reach at once the promised land and gather in the harvest, which had hitherto only been approached by long and laborious study. And so, for some time, a hard and fast line was drawn between the moral and the physical sciences; in fact, the very name of science was often denied to the latter.

The feud did not long subsist in its original intensity. The physical sciences proved conspicuously, by a brilliant series of discoveries and practical applications, that they contained a healthy germ of extraordinary fertility; it was impossible any longer to withhold from them recognition and respect. And even in other departments of science, conscientious investigators of facts soon protested against the over-bold flights of speculation. Still, it cannot be overlooked that the philosophy of Hegel and Schelling did exercise a beneficial influence; since their

time the attention of investigators in the moral sciences had been constantly and more keenly directed to the scope of those sciences, and to their intellectual contents, and therefore the great amount of labour bestowed on those systems has not been entirely thrown away.

We see, then, that in proportion as the experimental investigation of facts has recovered its importance in the moral sciences, the opposition between them and the physical sciences has become less and less marked. Yet we must not forget that, though this opposition was brought out in an unnecessarily exaggerated form by the Hegelian philosophy, it has its foundation in the nature of things, and must, sooner or later, make itself felt. It depends partly on the nature of the intellectual processes the two groups of sciences involve, partly, as their very names imply, on the subjects of which they treat. It is not easy for a scientific man to convey to a scholar or a jurist a clear idea of a complicated process of nature; he must demand of them a certain power of abstraction from the phenomena, as well as a certain skill in the use of geometrical and mechanical conceptions, in which it is difficult for them to follow him. On the other hand an artist or a theologian will perhaps find the natural philosopher too much inclined to mechanical and material explanations, which seem to them commonplace, and chilling to their feeling and enthusiasm. Nor will the scholar or the historian, who have some common ground with the theologian and the jurist, fare better with the natural philosopher. They will find him shockingly indifferent to literary treasures, perhaps even more indifferent than he ought to be to the history of his own science. In short, there is no denying that, while the moral sciences deal directly with the nearest and dearest interests of the human mind, and with the institutions it has brought into being, the natural sciences are con-

cerned with dead, indifferent matter, obviously indispensable for the sake of its practical utility, but apparently without any immediate bearing on the cultivation of the intellect.

It has been shown, then, that the sciences have branched out into countless ramifications, that there has grown up between different groups of them a real and deeply-felt opposition, that finally no single intellect can embrace the whole range, or even a considerable portion of it. Is it still reasonable to keep them together in one place of education? Is the union of the four Faculties to form one University a mere relic of the Middle Ages? Many valid arguments have been adduced for separating them. Why not dismiss the medical faculty to the hospitals of our great towns, the scientific men to the Polytechnic Schools, and form special seminaries for the theologians and jurists? Long may the German universities be preserved from such a fate! Then, indeed, would the connection between the different sciences be finally broken. How essential that connection is, not only from an university point of view, as tending to keep alive the intellectual energy of the country, but also on material grounds, to secure the successful application of that energy, will be evident from a few considerations.

First, then, I would say that union of the different Faculties is necessary to maintain a healthy equilibrium among the intellectual energies of students. Each study tries certain of our intellectual faculties more than the rest, and strengthens them accordingly by constant exercise. But any sort of one-sided development is attended with danger; it disqualifies us for using those faculties that are less exercised, and so renders us less capable of a general view; above all it leads us to overvalue ourselves. Anyone who has found himself much more suc-

cessful than others in some one department of intellectual labour, is apt to forget that there are many other things which they can do better than he can: a mistake—I would have every student remember—which is the worst enemy of all intellectual activity.

How many men of ability have forgotten to practise that criticism of themselves which is so essential to the student, and so hard to exercise, or have been completely crippled in their progress, because they have thought dry, laborious drudgery beneath them, and have devoted all their energies to the quest of brilliant theories and wonder-working discoveries! How many such men have become bitter misanthropes, and put an end to a melancholy existence, because they have failed to obtain among their fellows that recognition which must be won by labour and results, but which is ever withheld from mere self-conscious genius! And the more isolated a man is, the more liable is he to this danger; while, on the other hand, nothing is more inspiriting than to feel yourself forced to strain every nerve to win the admiration of men whom you, in your turn, must admire.

In comparing the intellectual processes involved in the pursuit of the several branches of science, we are struck by certain generic differences, dividing one group of sciences from another. At the same time it must not be forgotten that every man of conspicuous ability has his own special mental constitution, which fits him for one line of thought rather than another. Compare the work of two contemporary investigators even in closely-allied branches of science, and you will generally be able to convince yourself that the more distinguished the men are, the more clearly does their individuality come out, and the less qualified would either of them be to carry on the other's researches. To-day I can, of course, do

nothing more than characterise some of the most general of these differences.

I have already noticed the enormous mass of the materials accumulated by science. It is obvious that the organisation and arrangement of them must be proportionately perfect, if we are not to be hopelessly lost in the maze of erudition. One of the reasons why we can so far surpass our predecessors in each individual study is that they have shown us how to organise our knowledge.

This organisation consists, in the first place, of a mechanical arrangement of materials, such as is to be found in our catalogues, lexicons, registers, indexes, digests, scientific and literary annuals, systems of natural history, and the like. By these appliances thus much at least is gained, that such knowledge as cannot be carried about in the memory is immediately accessible to anyone who wants it. With a good lexicon a school-boy of the present day can achieve results in the interpretation of the classics, which an Erasmus, with the erudition of a lifetime, could hardly attain. Works of this kind form, so to speak, our intellectual principal, with the interest of which we trade; it is, so to speak, like capital invested in land. The learning buried in catalogues, lexicons, and indexes looks as bare and uninviting as the soil of a farm; the uninitiated cannot see or appreciate the labour and capital already invested there; to them the work of the ploughman seems infinitely dull, weary, and monotonous. But though the compiler of a lexicon or of a system of natural history must be prepared to encounter labour as weary and as obstinate as the ploughman's, yet it need not be supposed that his work is of a low type, or that it is by any means as dry and mechanical as it looks when we have it before us in black and white. In this, as in any other sort of scien-

tific work, it is necessary to discover every fact by careful observation, then to verify and collate them, and to separate what is important from what is not. All this requires a man with a thorough grasp, both of the object of the compilation, and of the matter and methods of the science; and for such a man every detail has its bearing on the whole, and its special interest. Otherwise dictionary-making would be the vilest drudgery imaginable.[1] That the influence of the progressive development of scientific ideas extends to these works is obvious from the constant demand for new lexicons, new natural histories, new digests, new catalogues of stars, all denoting advancement in the art of methodising and organising science.

But our knowledge is not to lie dormant in the shape of catalogues. The very fact that we must carry it about in black and white shows that our intellectual mastery of it is incomplete. It is not enough to be acquainted with the facts; scientific knowledge begins only when their laws and their causes are unveiled. Our materials must be worked up by a logical process; and the first step is to connect like with like, and to elaborate a general conception embracing them all. Such a conception, as the name implies, takes a number of single facts together, and stands as their representative in our mind. We call it a general conception, or the conception of a genus, when it embraces a number of existing objects; we call it a law when it embraces a series of incidents or occurrences. When, for example, I have made out that all mammals—that is, all warm-blooded, viviparous animals—breathe through lungs, have two chambers in the heart and at least three tympanal bones, I need no longer remember these anatomical peculiarities in the individual cases of the monkey, the dog, the horse, and the whale; the

[1] Condendaque lexica mandat damnatis.—Tr.

general rule includes a vast number of single instances, and represents them in my memory. When I enunciate the law of refraction, not only does this law embrace all cases of rays falling at all possible angles on a plane surface of water, and inform me of the result, but it includes all cases of rays of any colour incident on transparent surfaces of any form and any constitution whatsoever. This law, therefore, includes an infinite number of cases, which it would have been absolutely impossible to carry in one's memory. Moreover, it should be noticed that not only does this law include the cases which we ourselves or other men have already observed, but that we shall not hesitate to apply it to new cases, not yet observed, with absolute confidence in the reliability of our results. In the same way, if we were to find a new species of mammal, not yet dissected, we are entitled to assume, with a confidence bordering on a certainty, that it has lungs, two chambers in the heart, and three or more tympanal bones.

Thus, when we combine the results of experience by a process of thought, and form conceptions, whether general conceptions or laws, we not only bring our knowledge into a form in which it can be easily used and easily retained, but we actually enlarge it, inasmuch as we feel ourselves entitled to extend the rules and the laws we have discovered to all similar cases that may be hereafter presented to us.

The above-mentioned examples are of a class in which the mental process of combining a number of single cases so as to form conceptions is unattended by farther difficulties, and can be distinctly followed in all its stages. But in complicated cases it is not so easy completely to separate like facts from unlike, and to combine them into a clear, well-defined conception. Assume that we know a man to be ambitious; we shall perhaps be able to predict

with tolerable certainty that if he has to act under certain conditions, he will follow the dictates of his ambition, and decide on a certain line of action. But, in the first place, we cannot define with absolute precision what constitutes an ambitious man, or by what standard the intensity of his ambition is to be measured; nor, again, can we say precisely what degree of ambition must operate in order to impress the given direction on the actions of the man under those particular circumstances. Accordingly, we institute comparisons between the actions of the man in question, as far as we have hitherto observed them, and those of other men who in similar cases have acted as he has done, and we draw our inference respecting his future actions without being able to express either the major or the minor premiss in a clear, sharply-defined form—perhaps even without having convinced ourselves that our anticipation rests on such an analogy as I have described. In such cases our decision proceeds only from a certain psychological instinct, not from conscious reasoning, though in reality we have gone through an intellectual process identical with that which leads us to assume that a newly-discovered mammal has lungs.

This latter kind of induction, which can never be perfectly assimilated to forms of logical reasoning, nor pressed so far as to establish universal laws, plays a most important part in human life. The whole of the process by which we translate our sensations into perceptions depends upon it, as appears especially from the investigation of what are called illusions. For instance, when the retina of the eye is irritated by a blow, we imagine we see a light in our field of vision, because we have, throughout our lives, felt irritation in the optic nerves only when there was light in the field of vision, and have become accustomed to identify the sensations of those nerves with the presence of light in the field of vision.

Moreover, such is the complexity of the influences affecting the formation both of character in general and of the mental condition at any given moment, that this same kind of induction necessarily plays a leading part in the investigation of psychological processes. In fact, in ascribing to ourselves free-will, that is, full power to act as we please, without being subject to a stern inevitable law of causality, we deny *in toto* the possibility of referring at least one of the ways in which our mental activity expresses itself to a rigorous law.

We might possibly, in opposition to *logical induction* which reduces a question to clearly-defined universal propositions, call this kind of reasoning *æsthetic induction*, because it is most conspicuous in the higher class of works of art. It is an essential part of an artist's talent to reproduce by words, by form, by colour, or by music, the external indications of a character or a state of mind, and by a kind of instinctive intuition, uncontrolled by any definable rule, to seize the necessary steps by which we pass from one mood to another. If we do find that the artist has consciously worked after general rules and abstractions, we think his work poor and commonplace, and cease to admire. On the contrary, the works of great artists bring before us characters and moods with such a lifelikeness, with such a wealth of individual traits and such an overwhelming conviction of truth, that they almost seem to be more real than the reality itself, because all disturbing influences are eliminated.

Now if, after these reflections, we proceed to review the different sciences, and to classify them according to the method by which they must arrive at their results, we are brought face to face with a generic difference between the natural and the moral sciences. The natural sciences are for the most part in a position to reduce their inductions to sharply-defined general rules and principles;

the moral sciences, on the other hand, have, in by far the most numerous cases, to do with conclusions arrived at by psychological instinct. Philology, in so far as it is concerned with the interpretation and emendation of the texts handed down to us, must seek to feel out, as it were, the meaning which the author intended to express, and the accessory notions which he wished his words to suggest; and for that purpose it is necessary to start with a correct insight, both into the personality of the author, and into the genius of the language in which he wrote. All this affords scope for æsthetic, but not for strictly logical induction. It is only possible to pass judgment, if you have ready in your memory a great number of similar facts, to be instantaneously confronted with the question you are trying to solve. Accordingly, one of the first requisites for studies of this class is an accurate and ready memory. Many celebrated historians and philologists have, in fact, astounded their contemporaries by their extraordinary strength of memory. Of course memory alone is insufficient without a knack of everywhere discovering real resemblance, and without a delicately and fully trained insight into the springs of human action; while this again is unattainable without a certain warmth of sympathy and an interest in observing the working of other men's minds. Intercourse with our fellow-men in daily life must lay the foundation of this insight, but the study of history and art serves to make it richer and completer, for there we see men acting under comparatively unusual conditions, and thus come to appreciate the full scope of the energies which lie hidden in our breasts.

None of this group of sciences, except grammar, lead us, as a rule, to frame and enunciate general laws, valid under all circumstances. The laws of grammar are a product of the human will, though they can hardly be

said to have been framed deliberately, but rather to have grown up gradually, as they were wanted. Accordingly, they present themselves to a learner rather in the form of commands, that is, of laws imposed by external authority.

With these sciences theology and jurisprudence are naturally connected. In fact, certain branches of history and philology serve both as stepping-stones and as handmaids to them. The general laws of theology and jurisprudence are likewise commands, laws imposed by external authority to regulate, from a moral or juridical point of view, the actions of mankind; not laws which, like those of nature, contain generalisations from a vast multitude of facts. At the same time the application of a grammatical, legal, moral, or theological rule is couched, like the application of a law of nature to a particular case, in the forms of logical inference. The rule forms the major premiss of the syllogism, while the minor must settle whether the case in question satisfies the conditions to which the rule is intended to apply. The solution of this latter problem, whether in grammatical analysis, where the meaning of a sentence is to be evolved, or in the legal criticism of the credibility of the facts alleged, of the intentions of the parties, or of the meaning of the documents they have put into court, will, in most cases, be again a matter of psychological insight. On the other hand, it should not be forgotten that both the syntax of fully-developed languages and a system of jurisprudence gradually elaborated, as ours has been, by the practice of more than 2,000 years,[1] have reached a high pitch of logical completeness and consistency; so that, speaking generally, the cases which do not obviously fall under

[1] It should be remembered that the Roman law, which has only partially and indirectly influenced English practice, is the recognised basis of German jurisprudence.—Tr.

some one or other of the laws actually laid down are quite exceptional. Such exceptions there will always be, for the legislation of man can never have the absolute consistency and perfection of the laws of nature. In such cases there is no course open but to try and guess the intention of the legislator; or, if needs be, to supplement it after the analogy of his decisions in similar cases.

Grammar and jurisprudence have a certain advantage as means of training the intellect, inasmuch as they tax pretty equally all the intellectual powers. On this account secondary education among modern European nations is based mainly upon the grammatical study of foreign languages. The mother-tongue and modern foreign languages, when acquired solely by practice, do not call for any conscious logical exercise of thought, though we may cultivate by means of them an appreciation for artistic beauty of expression. The two classical languages, Latin and Greek, have, besides their exquisite logical subtlety and æsthetic beauty, an additional advantage, which they seem to possess in common with most ancient and original languages—they indicate accurately the relations of words and sentences to each other by numerous and distinct inflexions. Languages are, as it were, abraded by long use; grammatical distinctions are cut down to a minimum for the sake of brevity and rapidity of expression, and are thus made less and less definite, as is obvious from the comparison of any modern European language with Latin; in English the process has gone further than in any other. This seems to me to be really the reason why the modern languages are far less fitted than the ancient for instruments of education.[1]

[1] Those to whom German is not a foreign tongue may, perhaps, be permitted to hold different views on the efficacy of modern languages in education.—Tr.

As grammar is the staple of school education, legal studies are used, and rightly, as a means of training persons of maturer age, even when not specially required for professional purposes.

We now come to those sciences which, in respect of the kind of intellectual labour they require, stand at the opposite end of the series to philology and history; namely, the natural and physical sciences. I do not mean to say that in many branches even of these sciences an instinctive appreciation of analogies and a certain artistic sense have no part to play. On the contrary, in natural history the decision which characteristics are to be looked upon as important for classification, and which as unimportant, what divisions of the animal and vegetable kingdoms are more natural than others, is really left to an instinct of this kind, acting without any strictly definable rule. And it is a very suggestive fact that it was an artist, Goethe, who gave the first impulse to the researches of comparative anatomy into the analogy of corresponding organs in different animals, and to the parallel theory of the metamorphosis of leaves in the vegetable kingdom; and thus, in fact, really pointed out the direction which the science has followed ever since. But even in those departments of science where we have to do with the least understood vital processes it is, speaking generally, far easier to make out general and comprehensive ideas and principles, and to express them in definite language, than in cases where we must base our judgment on the analysis of the human mind. It is only when we come to the experimental sciences to which mathematics are applied, and especially when we come to pure mathematics, that we see the peculiar characteristics of the natural and physical sciences fully brought out.

The essential *differentia* of these sciences seems to me to consist in the comparative ease with which the indi-

vidual results of observation and experiment are combined under general laws of unexceptionable validity and of an extraordinarily comprehensive character. In the moral sciences, on the other hand, this is just the point where insuperable difficulties are encountered. In mathematics the general propositions which, under the name of axioms, stand at the head of the reasoning, are so few in number, so comprehensive, and so immediately obvious, that no proof whatever is needed for them. Let me remind you that the whole of algebra and arithmetic is developed out of the three axioms:

'Things which are equal to the same things are equal to one another.'

'If equals be added to equals, the wholes are equal.'

'If unequals be added to equals, the wholes are unequal.'

And the axioms of geometry and mechanics are not more numerous. The sciences we have named are developed out of these few axioms by a continual process of deduction from them in more and more complicated cases. Algebra, however, does not confine itself to finding the sum of the most heterogeneous combinations of a finite number of magnitudes, but in the higher analysis it teaches us to sum even infinite series, the terms of which increase or diminish according to the most various laws; to solve, in fact, problems which could never be completed by direct addition. An instance of this kind shows us the conscious logical activity of the mind in its purest and most perfect form. On the one hand we see the laborious nature of the process, the extreme caution with which it is necessary to advance, the accuracy required to determine exactly the scope of such universal principles as have been attained, the difficulty of forming and understanding abstract conceptions. On the other hand, we gain confidence in the certainty, the range, and the fertility of this kind of intellectual work.

The fertility of the method comes out more strikingly in applied mathematics, especially in mathematical physics, including, of course, physical astronomy. From the time when Newton discovered, by analysing the motions of the planets on mechanical principles, that every particle of ponderable matter in the universe attracts every other particle with a force varying inversely as the square of the distance, astronomers have been able, in virtue of that one law of gravitation, to calculate with the greatest accuracy the movements of the planets to the remotest past and the most distant future, given only the position, velocity, and mass of each body of our system at any one time. More than that, we recognise the operation of this law in the movements of double stars, whose distances from us are so great that their light takes years to reach us; in some cases, indeed, so great that all attempts to measure them have failed.

This discovery of the law of gravitation and its consequences is the most imposing achievement that the logical power of the human mind has hitherto performed. I do not mean to say that there have not been men who in power of abstraction have equalled or even surpassed Newton and the other astronomers, who either paved the way for his discovery, or have carried it out to its legitimate consequences; but there has never been presented to the human mind such an admirable subject as those involved and complex movements of the planets, which hitherto had served merely as food for the astrological superstitions of ignorant star-gazers, and were now reduced to a single law, capable of rendering the most exact account of the minutest detail of their motions.

The principles of this magnificent discovery have been successfully applied to several other physical sciences, among which physical optics and the theory of electricity and magnetism are especially worthy of notice. The ex-

perimental sciences have one great advantage over the natural sciences in the investigation of general laws of nature: they can change at pleasure the conditions under which a given result takes place, and can thus confine themselves to a small number of characteristic instances, in order to discover the law. Of course its validity must then stand the test of application to more complex cases. Accordingly the physical sciences, when once the right methods have been discovered, have made proportionately rapid progress. Not only have they allowed us to look back into primæval chaos, where nebulous masses were forming themselves into suns and planets, and becoming heated by the energy of their contraction; not only have they permitted us to investigate the chemical constituents of the solar atmosphere and of the remotest fixed stars, but they have enabled us to turn the forces of surrounding nature to our own uses and to make them the ministers of our will.

Enough has been said to show how widely the intellectual processes involved in this group of sciences differ, for the most part, from those required by the moral sciences. The mathematician need have no memory whatever for detached facts, the physicist hardly any. Hypotheses based on the recollection of similar cases may, indeed, be useful to guide one into the right track, but they have no real value till they have led to a precise and strictly defined law. Nature does not allow us for a moment to doubt that we have to do with a rigid chain of cause and effect, admitting of no exceptions. Therefore to us, as her students, goes forth the mandate to labour on till we have discovered unvarying laws; till then we dare not rest satisfied, for then only can our knowledge grapple victoriously with time and space and the forces of the universe.

The iron labour of conscious logical reasoning demands great perseverance and great caution; it moves on but

slowly, and is rarely illuminated by brilliant flashes of genius. It knows little of that facility with which the most varied instances come thronging into the memory of the philologist or the historian. Rather is it an essential condition of the methodical progress of mathematical reasoning that the mind should remain concentrated on a single point, undisturbed alike by collateral ideas on the one hand, and by wishes and hopes on the other, and moving on steadily in the direction it has deliberately chosen. A celebrated logician, Mr. John Stuart Mill, expresses his conviction that the inductive sciences have of late done more for the advance of logical methods than the labours of philosophers properly so called. One essential ground for such an assertion must undoubtedly be that in no department of knowledge can a fault in the chain of reasoning be so easily detected by the incorrectness of the results as in those sciences in which the results of reasoning can be most directly compared with the facts of nature.

Though I have maintained that it is in the physical sciences, and especially in such branches of them as are treated mathematically, that the solution of scientific problems has been most successfully achieved, you will not, I trust, imagine that I wish to depreciate other studies in comparison with them. If the natural and physical sciences have the advantage of great perfection in form, it is the privilege of the moral sciences to deal with a richer material, with questions that touch more nearly the interests and the feelings of men, with the human mind itself, in fact, in its motives and the different branches of its activity. They have, indeed, the loftier and the more difficult task, but yet they cannot afford to lose sight of the example of their rivals, which, in form at least, have, owing to the more ductile nature of their materials, made greater progress. Not only have they something to learn from them in point of

method, but they may also draw encouragement from the greatness of their results. And I do think that our age has learnt many lessons from the physical sciences. The absolute, unconditional reverence for facts, and the fidelity with which they are collected, a certain distrustfulness of appearances, the effort to detect in all cases relations of cause and effect, and the tendency to assume their existence, which distinguish our century from preceding ones, seem to me to point to such an influence.

I do not intend to go deeply into the question how far mathematical studies, as the representatives of conscious logical reasoning, should take a more important place in school education. But it is, in reality, one of the questions of the day. In proportion as the range of science extends, its system and organisation must be improved, and it must inevitably come about that individual students will find themselves compelled to go through a stricter course of training than grammar is in a position to supply. What strikes me in my own experience of students who pass from our classical schools to scientific and medical studies, is first, a certain laxity in the application of strictly universal laws. The grammatical rules, in which they have been exercised, are for the most part followed by long lists of exceptions; accordingly they are not in the habit of relying implicitly on the certainty of a legitimate deduction from a strictly universal law. Secondly, I find them for the most part too much inclined to trust to authority, even in cases where they might form an independent judgment. In fact, in philological studies, inasmuch as it is seldom possible to take in the whole of the premisses at a glance, and inasmuch as the decision of disputed questions often depends on an æsthetic feeling for beauty of expression, and for the genius of the language, attainable only by long training, it must often happen that the

student is referred to authorities even by the best teachers. Both faults are traceable to a certain indolence and vagueness of thought, the sad effects of which are not confined to subsequent scientific studies. But certainly the best remedy for both is to be found in mathematics, where there is absolute certainty in the reasoning, and no authority is recognised but that of one's own intelligence.

So much for the several branches of science considered as exercises for the intellect, and as supplementing each other in that respect. But knowledge is not the sole object of man upon earth. Though the sciences arouse and educate the subtlest powers of the mind, yet a man who should study simply for the sake of knowing, would assuredly not fulfil the purpose of his existence. We often see men of considerable endowments, to whom their good or bad fortune has secured a comfortable livelihood or good social position, without giving them, at the same time, ambition or energy enough to make them work, dragging out a weary, unsatisfied existence, while all the time they fancy they are following the noblest aim of life by constantly devoting themselves to the increase of their knowledge, and the cultivation of their minds. Action alone gives a man a life worth living; and therefore he must aim either at the practical application of his knowledge, or at the extension of the limits of science itself. For to extend the limits of science is really to work for the progress of humanity. Thus we pass to the second link, uniting the different sciences, the connection, namely, between the subjects of which they treat.

Knowledge is power. Our age, more than any other, is in a position to demonstrate the truth of this maxim. We have taught the forces of inanimate nature to minister to the wants of human life and the designs of

the human intellect. The application of steam has multiplied our physical strength a million-fold; weaving and spinning machines have relieved us of labours, the only merit of which consisted in a deadening monotony. The intercourse between men, with its far-reaching influence on material and intellectual progress, has increased to an extent of which no one could have even dreamed within the lifetime of the older among us. But it is not merely on the machines by which our powers are multiplied; not merely on rifled cannon, and armour-plated ships; not merely on accumulated stores of money and the necessaries of life, that the power of a nation rests; though these things have exercised so unmistakeable an influence, that even the proudest and most obstinate despotisms of our times have been forced to think of removing restrictions on industry, and of conceding to the industrious middle classes a due voice in their counsels. But political organisation, the administration of justice, and the moral discipline of individual citizens are no less important conditions of the preponderance of civilised nations; and so surely as a nation remains inaccessible to the influences of civilisation in these respects, so surely is it on the high road to destruction. The several conditions of national prosperity act and react on each other; where the administration of justice is uncertain, where the interests of the majority cannot be asserted by legitimate means, the development of the national resources, and of the power depending upon them, is impossible; nor again, is it possible to make good soldiers except out of men who have learnt under just laws to educate the sense of honour that characterises an independent man, certainly not out of those who have lived the submissive slaves of a capricious tyrant.

Accordingly every nation is interested in the progress of knowledge on the simple ground of self-preservation,

even were there no higher wants of an ideal character to be satisfied; and not merely in the development of the physical sciences, and their technical application, but also in the progress of legal, political, and moral sciences, and of the accessory historical and philological studies. No nation which would be independent and influential can afford to be left behind in the race. Nor has this escaped the notice of the cultivated peoples of Europe. Never before was so large a part of the public resources devoted to universities, schools, and scientific institutions. We in Heidelberg have this year occasion to congratulate ourselves on another rich endowment granted by our government and our parliament.

I was speaking, at the beginning of my address, of the increasing division of labour and the improved organisation among scientific workers. In fact, men of science form, as it were, an organised army, labouring on behalf of the whole nation, and generally under its direction and at its expense, to augment the stock of such knowledge as may serve to promote industrial enterprise, to increase wealth, to adorn life, to improve political and social relations, and to further the moral development of individual citizens. After the immediate practical results of their work we forbear to inquire; that we leave to the uninstructed. We are convinced that whatever contributes to the knowledge of the forces of nature or the powers of the human mind is worth cherishing, and may, in its own due time, bear practical fruit, very often where we should least have expected it. Who, when Galvani touched the muscles of a frog with different metals, and noticed their contraction, could have dreamt that eighty years afterwards, in virtue of the self-same process, whose earliest manifestations attracted his attention in his anatomical researches, all Europe would be traversed with wires, flashing intelligence from Madrid

to St. Petersburg with the speed of lightning? In the hands of Galvani, and at first even in Volta's, electrical currents were phenomena capable of exerting only the feeblest forces, and could not be detected except by the most delicate apparatus. Had they been neglected, on the ground that the investigation of them promised no immediate practical result, we should now be ignorant of the most important and most interesting of the links between the various forces of nature. When young Galileo, then a student at Pisa, noticed one day during divine service a chandelier swinging backwards and forwards, and convinced himself, by counting his pulse, that the duration of the oscillations was independent of the arc through which it moved, who could know that this discovery would eventually put it in our power, by means of the pendulum, to attain an accuracy in the measurement of time till then deemed impossible, and would enable the storm-tossed seaman in the most distant oceans to determine in what degree of longitude he was sailing?

Whoever, in the pursuit of science, seeks after immediate practical utility, may generally rest assured that he will seek in vain. All that science can achieve is a perfect knowledge and a perfect understanding of the action of natural and moral forces. Each individual student must be content to find his reward in rejoicing over new discoveries, as over new victories of mind over reluctant matter, or in enjoying the æsthetic beauty of a well-ordered field of knowledge, where the connection and the filiation of every detail is clear to the mind, and where all denotes the presence of a ruling intellect; he must rest satisfied with the consciousness that he too has contributed something to the increasing fund of knowledge on which the dominion of man over all the forces hostile to intelligence reposes. He will, indeed, not always be permitted to expect from his fellow-men appreciation and reward

adequate to the value of his work. It is only too true, that many a man to whom a monument has been erected after his death, would have been delighted to receive during his lifetime a tenth part of the money spent in doing honour to his memory At the same time, we must acknowledge that the value of scientific discoveries is now far more fully recognised than formerly by public opinion, and that instances of the authors of great advances in science starving in obscurity have become rarer and rarer. On the contrary, the governments and peoples of Europe have, as a rule, admitted it to be their duty to recompense distinguished achievements in science by appropriate appointments or special rewards.

The sciences have then, in this respect, all one common aim, to establish the supremacy of intelligence over the world: while the moral sciences aim directly at making the resources of intellectual life more abundant and more interesting, and seek to separate the pure gold of Truth from alloy, the physical sciences are striving indirectly towards the same goal, inasmuch as they labour to make mankind more and more independent of the material restraints that fetter their activity. Each student works in his own department, he chooses for himself those tasks for which he is best fitted by his abilities and his training. But each one must be convinced that it is only in connection with others that he can further the great work, and that therefore he is bound, not only to investigate, but to do his utmost to make the results of his investigation completely and easily accessible. If he does this, he will derive assistance from others, and will in his turn be able to render them his aid. The annals of science abound in evidence how such mutual services have been exchanged, even between departments of science apparently most remote. Historical chronology is essentially based on astronomical calculations of eclipses, accounts of which

are preserved in ancient histories. Conversely, many of the important data of astronomy—for instance, the invariability of the length of the day, and the periods of several comets—rest upon ancient historical notices. Of late years, physiologists, especially Brücke, have actually undertaken to draw up a complete system of all the vocables that can be produced by the organs of speech, and to base upon it propositions for an universal alphabet, adapted to all human languages. Thus physiology has entered the service of comparative philology, and has already succeeded in accounting for many apparently anomalous substitutions, on the ground that they are governed, not as hitherto supposed, by the laws of euphony, but by similarity between the movements of the mouth that produce them. Again, comparative philology gives us information about the relationships, the separations and the migrations of tribes in prehistoric times, and of the degree of civilisation which they had reached at the time when they parted. For the names of objects to which they had already learnt to give distinctive appellations reappear as words common to their later languages. So that the study of languages actually gives us historical data for periods respecting which no other historical evidence exists.[1] Yet again I may notice the help which not only the sculptor, but the archæologist, concerned with the investigation of ancient statues, derives from anatomy. And if I may be permitted to refer to my own most recent studies, I would mention that it is possible, by reference to physical acoustics and to the physiological theory of the sensation of hearing, to account for the elementary principles on which our musical system is constructed, a problem essentially within the sphere of æsthetics. In fact, it is a general principle that the physiology of the organs of sense is

[1] See, for example, Mommsen's *Rome*, Book I. ch. ii.—Tr.

most intimately connected with psychology, inasmuch as physiology traces in our sensations the results of mental processes which do not fall within the sphere of conciousness, and must therefore have remained inaccessible to us.

I have been able to quote only some of the most striking instances of this interdependence of different sciences, and such as could be explained in a few words. Naturally, too, I have tried to choose them from the most widely-separated sciences. But far wider is of course the influence which allied sciences exert upon each other. Of that I need not speak, for each of you knows it from his own experience.

In conclusion, I would say, let each of us think of himself, not as a man seeking to gratify his own thirst for knowledge, or to promote his own private advantage, or to shine by his own abilities, but rather as a fellow-labourer in one great common work bearing upon the highest interests of humanity. Then assuredly we shall not fail of our reward in the approval of our own conscience and the esteem of our fellow-citizens. To keep up these relations between all searchers after truth and all branches of knowledge, to animate them all to vigorous co-operation towards their common end, is the great office of the Universities. Therefore is it necessary that the four Faculties should ever go hand in hand, and in this conviction will we strive, so far as in us lies, to press onward to the fulfilment of our great mission.

ON

GOETHE'S SCIENTIFIC RESEARCHES.

A LECTURE DELIVERED BEFORE THE GERMAN SOCIETY OF KÖNIGSBERG, IN THE SPRING OF 1853.

It could not but be that Goethe, whose comprehensive genius was most strikingly apparent in that sober clearness with which he grasped and reproduced with lifelike freshness the realities of nature and human life in their minutest details, should, by those very qualities of his mind, be drawn towards the study of physical science. And in that department, he was not content with acquiring what others could teach him, but he soon attempted, as so original a mind was sure to do, to strike out an independent and a very characteristic line of thought. He directed his energies, not only to the descriptive, but also to the experimental sciences; the chief results being his botanical and osteological treatises on the one hand, and his theory of colour on the other. The first germs of these researches belong for the most part to the last decade of the eighteenth century, though some of them were not completed nor published till later. Since that time science has not only made great progress, but has widely extended its range. It has assumed in some respects an entirely new aspect, it has opened out

new fields of research and undergone many changes in its theoretical views. I shall attempt in the following Lecture to sketch the relation of Goethe's researches to the present stand-point of science, and to bring out the guiding idea that is common to them all.

The peculiar character of the descriptive sciences—botany, zoology, anatomy, and the like—is a necessary result of the work imposed upon them. They undertake to collect and sift an enormous mass of facts, and, above all, to bring them into a logical order or system. Up to this point their work is only the dry task of a lexicographer; their system is nothing more than a muniment-room in which the accumulation of papers is so arranged that any one can find what he wants at any moment. The more intellectual part of their work and their real interest only begins when they attempt to feel after the scattered traces of law and order in the disjointed, heterogeneous mass, and out of it to construct for themselves an orderly system, accessible at a glance, in which every detail has its due place, and gains additional interest from its connection with the whole.

In such studies, both the organising capacity and the insight of our poet found a congenial sphere—the epoch was moreover propitious to him. He found ready to his hand a sufficient store of logically arranged materials in botany and comparative anatomy, copious and systematic enough to admit of a comprehensive view, and to indicate the way to some happy glimpse of an all-pervading law; while his contemporaries, if they made any efforts in this direction, wandered without a compass, or else they were so absorbed in the dry registration of facts, that they scarcely ventured to think of anything beyond. It was reserved for Goethe to introduce two ideas of infinite fruitfulness.

The first was the conception that the differences in the

anatomy of different animals are to be looked upon as variations from a common phase or type, induced by differences of habit, locality, or food. The observation which led him to this fertile conception was by no means a striking one; it is to be found in a monograph on the intermaxillary bone, written as early as 1786. It was known that in most vertebrate animals (that is, mammalia, birds, amphibia, and fishes) the upper jaw consists of two bones, the upper jaw-bone and the intermaxillary bone. The former always contains in the mammalia the molar and the canine teeth, the latter the incisors. Man, who is distinguished from all other animals by the absence of the projecting snout, has, on the contrary, on each side only one bone, the upper jaw-bone, containing all the teeth. This being so, Goethe discovered in the *human* skull faint traces of the sutures, which in animals unite the upper and middle jaw-bones, and concluded from it that man had originally possessed an intermaxillary bone, which had subsequently coalesced with the upper jaw-bone. This obscure fact opened up to him a source of the most intense interest in the field of osteology, generally so much decried as the driest of studies. That details of structure should be the same in man and in animals when the parts continue to perform similar functions had involved nothing extraordinary. In fact, Camper had already attempted, on this principle, to trace similarities of structure even between man and fishes. But the persistence of this similarity, at least in a rudimentary form, even in a case when it evidently does not correspond to any of the requirements of the complete human structure, and consequently needs to be adapted to them by the coalescence of two parts originally separate, was what struck Goethe's far-seeing eye, and suggested to him a far more comprehensive view than had hitherto been taken. Further studies soon convinced

him of the universality of his newly-discovered principle, so that in 1795 and 1796 he was able to define more clearly the idea that had struck him in 1786, and to commit it to writing in his 'Sketch of a General Introduction to Comparative Anatomy.' He there lays down with the utmost confidence and precision, that all differences in the structure of animals must be looked upon as variations of a single primitive type, induced by the coalescence, the alteration, the increase, the diminution, or even the complete removal of single parts of the structure; the very principle, in fact, which has become the leading idea of comparative anatomy in its present stage. Nowhere has it been better or more clearly expressed than in Goethe's writings. Subsequent authorities have made but few essential alterations in his theory. The most important of these is, that we no longer undertake to construct a common type for the whole animal kingdom, but are content with one for each of Cuvier's great divisions. The industry of Goethe's successors has accumulated a well-sifted stock of facts, infinitely more copious than what he could command, and has followed up successfully into the minutest details what he could only indicate in a general way.

The second leading conception which science owes to Goethe enunciated the existence of an analogy between the different parts of one and the same organic being, similar to that which we have just pointed out as subsisting between corresponding parts of different species. In most organisms we see a great repetition of single parts. This is most striking in the vegetable kingdom: each plant has a great number of similar stem leaves, similar petals, similar stamens, and so on. According to Goethe's own account, the idea first occurred to him while looking at a fan-palm at Padua. He was struck by the immense variety of changes of form which the suc-

cessively-developed stem-leaves exhibit, by the way in which the first simple root leaflets are replaced by a series of more and more divided leaves, till we come to the most complicated.

He afterwards succeeded in discovering the transformation of stem-leaves into sepals and petals, and of sepals and petals into stamens, nectaries, and ovaries, and thus he was led to the doctrine of the metamorphosis of plants, which he published in 1790. Just as the anterior extremity of vertebrate animals takes different forms, becoming in man and in apes an arm, in other animals a paw with claws, or a forefoot with a hoof, or a fin, or a wing, but always retains the same divisions, the same position, and the same connection with the trunk, so the leaf appears as a cotyledon, stem-leaf, sepal, petal, stamen, nectary, ovary, &c., all resembling each other to a certain extent in origin and composition, and even capable, under certain unusual conditions, of passing from one form into the other, as, for example, may be seen by any one who looks carefully at a full-blown rose, where some of the stamens are completely, some of them partially, changed into petals. This view of Goethe's, like the other, is now completely adopted into science, and enjoys the universal assent of botanists, though of course some details are still matters of controversy, as, for instance, whether the bud is a single leaf or a branch.

In the animal kingdom, the composition of an individual out of several similar parts is very striking in the great sub-kingdom of the articulata—for example, in insects and worms. The larva of an insect, or the caterpillar of a butterfly, consists of a number of perfectly similar segments; only the first and last of them differ, and that but slightly, from the others. After their transformation into perfect insects, they furnish clear and simple exemplifications of the view which Goethe had

grasped in his doctrine of the metamorphosis of plants, the development, namely, of apparently very dissimilar forms from parts originally alike. The posterior segments retain their original simple form; those of the breast-plate are drawn closely together, and develop feet and wings; while those of the head develop jaws and feelers; so that in the perfect insect, the original segments are recognised only in the posterior part of the body. In the vertebrata, again, a repetition of similar parts is suggested by the vertebral column, but has ceased to be observable in the external form. A fortunate glance at a broken sheep's skull, which Goethe found by accident on the sand of the Lido at Venice, suggested to him that the skull itself consisted of a series of very much altered vertebræ. At first sight, no two things can be more unlike than the broad uniform cranial cavity of the mammalia, inclosed by smooth plates, and the narrow cylindrical tube of the spinal marrow, composed of short, massy, jagged bones. It was a bright idea to detect the transformation in the skull of a mammal; the similarity is more striking in the amphibia and fishes. It should be added that Goethe left this idea unpublished for a long time, apparently because he was not quite sure how it would be received. Meantime, in 1806, the same idea occurred to Oken, who introduced it to the scientific world, and afterwards disputed with Goethe the priority of discovery. In fact, Goethe had waited till 1817, when the opinion had begun to find adherents, and then declared that he had had it in his mind for thirty years. Up to the present day, the number and composition of the vertebræ of the skull are a subject of controversy, but the principle has maintained its ground.

Goethe's views, however, on the existence of a common type in the animal kingdom do not seem to have exercised any direct influence on the progress of science. The

doctrine of the metamorphosis of plants was introduced into botany as his distinct and recognised property; but his views on osteology were at first disputed by anatomists, and only subsequently attracted attention when the science had, apparently on independent grounds, found its way to the same discovery. He himself complains that his first ideas of a common type had encountered nothing but contradiction and scepticism at the time when he was working them out in his own mind, and that even men of the freshest and most original intellect, like the two Von Humboldts, had listened to them with something like impatience. But it is almost a matter of course that in any natural or physical science, theoretical ideas attract the attention of its cultivators only when they are advanced in connection with the whole of the evidence on which they rest, and thus justify their title to recognition. Be that as it may, Goethe is entitled to the credit of having caught the first glimpse of the guiding ideas to which the sciences of botany and anatomy were tending, and by which their present form is determined.

But great as is the respect which Goethe has secured by his achievements in the descriptive natural sciences, the denunciation heaped by all physicists on his researches in their department, and especially on his 'theory of colour,' is at least as uncompromising. This is not the place to plunge into the controversy that raged on the subject, and so I shall only attempt to state clearly the points at issue, and to explain what principle was involved, and what is the latent significance of the dispute.

To this end it is of some importance to go back to the history of the origin of the theory, and to its simplest form, because at that stage of the controversy the points at issue are obvious, and admit of easy and dis-

tinct statement, unencumbered by disputes about the correctness of detached facts and complicated theories.

Goethe himself describes very gracefully, in the confession at the end of his 'Theory of Colour,' how he came to take up the subject. Finding himself unable to grasp the æsthetic principles involved in effects of colour, he resolved to resume the study of the physical theory, which he had been taught at the university, and to repeat for himself the experiments connected with it. With that view he borrowed a prism of Hofrath Bütter, of Jena, but was prevented by other occupations from carrying out his plan, and kept it by him for a long time unused. The owner of the prism, a very orderly man, after several times asking in vain, sent a messenger with instructions to bring it back directly. Goethe took it out of the case, and thought he would take one more peep through it. To make certain of seeing something, he turned it towards a long white wall, under the impression that as there was plenty of light there he could not fail to see a brilliant example of the resolution of light into different colours; a supposition, by the way, which shows how little Newton's theory of the phenomena was then present to his mind. Of course he was disappointed. On the white wall he saw no colours; they only appeared where it was bounded by darker objects. Accordingly he made the observation—which, it should be added, is fully accounted for by Newton's theory—that colour can only be seen through a prism where a dark object and a bright one have the same boundary. Struck by this observation, which was quite new to him, and convinced that it was irreconcilable with Newton's theory, he induced the owner of the prism to relent, and devoted himself to the question with the utmost zeal and interest. He prepared sheets of paper with black and white spaces, and studied the phenomenon under every variety of condition, until he thought he had

sufficiently proved his rules. He next attempted to explain his supposed discovery to a neighbour, who was a physicist, and was disagreeably surprised to be assured by him that the experiments were well known, and fully accounted for in Newton's theory. Every other natural philosopher whom he consulted told him exactly the same, including even the brilliant Lichtenberg, whom he tried for a long time to convert, but in vain. He studied Newton's writings, and fancied he had found some fallacies in them which accounted for the error. Unable to convince any of his acquaintances, he at last resolved to appear before the bar of public opinion, and in 1791 and 1792 published the first and second parts of his 'Contributions to Physical Optics.'

In that work he describes the appearances presented by white discs on a black ground, black discs on a white ground, and coloured discs on a black or white ground, when examined through a prism. As to the results of the experiments there is no dispute whatever between him and the physicists. He describes the phenomena he saw with great truth to nature; the style is lively, and the arrangement such as to make a conspectus of them easy and inviting; in short, in this as in all other cases where facts are to be described, he proves himself a master. At the same time he expresses his conviction that the facts he has adduced are calculated to refute Newton's theory. There are two points especially which he considers fatal to it: first, that the centre of a broad white surface remains white when seen through a prism; and secondly, that even a black streak on a white ground can be entirely decomposed into colours.

Newton's theory is based on the hypothesis that there exists light of different kinds, distinguished from one another by the sensation of colour which they produce in the eye. Thus there is red, orange, yellow, green, blue,

and violet light, and light of all intermediate colours. Different kinds of light, or differently coloured lights, produce, when mixed, derived colours, which to a certain extent resemble the original colours from which they are derived; to a certain extent form new tints. White is a mixture of all the before-named colours in certain definite proportions. But the primitive colours can always be reproduced by analysis from derived colours, or from white, while themselves incapable of analysis or change. The cause of the colours of transparent and opaque bodies is, that when white light falls upon them they destroy some of its constituents and send to the eye other constituents, but no longer mixed in the right proportions to produce white light. Thus a piece of red glass looks red, because it transmits only red rays. Consequently all colour is derived solely from a change in the proportions in which light is mixed, and is, therefore, a property of light, not of the coloured bodies, which only furnish an occasion for its manifestation.

A prism refracts transmitted light; that is to say, deflects it so that it makes a certain angle with its original direction; the rays of simple light of different colours have, according to Newton, different refrangibilities, and therefore, after refraction in the prism, pursue different courses and separate from each other. Accordingly a luminous point of infinitely small dimensions appears, when seen through the prism, to be first displaced, and secondly, extended into a coloured line, the so-called prismatic spectrum, which shows what are called the primary colours in the order above-named. If, however, you look at a broader luminous surface, the spectra of the points near the middle are superposed, as may be seen from a simple geometrical investigation, in such proportions as to give white light, except at the edges, where certain of the colours are free. This white surface appears displaced, as the luminous point did: but instead of being

coloured throughout, it has on one side a margin of blue and violet, on the other a margin of red and yellow. A black patch between two bright surfaces may be entirely covered by their coloured edges; and when these spectra meet in the middle, the red of the one and the violet of the other combine to form purple. Thus the colours into which, at first sight, it seems as if the black were analysed are in reality due, not to the black strip, but to the white on each side of it.

It is evident that at the first moment Goethe did not recollect Newton's theory well enough to be able to find out the physical explanation of the facts I have just glanced at. It was afterwards laid before him again and again, and that in a thoroughly intelligible form, for he speaks about it several times in terms that show he understood it quite correctly. But he is still so dissatisfied with it, that he persists in his assertion that the facts just cited are of a nature to convince any one who observes them of the absolute incorrectness of Newton's theory. Neither here nor in his later controversial writings does he ever clearly state in what he conceives the insufficiency of the explanation to consist. He merely repeats again and again that it is quite absurd. And yet I cannot see how any one, whatever his views about colour, can deny that the theory is perfectly consistent with itself; and that if the hypothesis from which it starts be granted, it explains the observed facts completely and even simply. Newton himself mentions these spurious spectra in several passages of his optical works, without going into any special elucidation of the point, considering, of course, that the explanation follows at once from his hypothesis. And he seems to have had good reason to think so; for Goethe no sooner began to call the attention of his scientific friends to the phenomena, than all with one accord, as he himself tells us, met his difficulties with this explanation from Newton's principles, which, though not actually in his

writings, instantly suggested itself to every one who knew them.

A reader who tries to realise attentively and thoroughly every step in this part of the controversy is apt to experience at this point an uncomfortable, almost a painful feeling to see a man of extraordinary abilities persistently declaring that there is an obvious absurdity lurking in a few inferences apparently quite clear and simple. He searches and searches, and at last unable, with all his efforts, to find any such absurdity, or even the appearance of it, he gets into a state of mind in which his own ideas are, so to speak, crystallised. But it is just this obvious, flat contradiction that makes Goethe's point of view in 1792 so interesting and so important. At this point he has not as yet developed any theory of his own; there is nothing under discussion but a few easily-grasped facts, as to the correctness of which both parties are agreed, and yet both hold distinctly opposite views; neither of them even understands what his opponent is driving at. On the one side are a number of physicists, who, by a long series of the ablest investigations, the most elaborate calculations, and the most ingenious inventions, have brought optics to such perfection, that it, and it alone, among the physical sciences, was beginning almost to rival astronomy in accuracy. Some of them have made the phenomena the subject of direct investigation; all of them, thanks to the accuracy with which it is possible to calculate beforehand the result of every variety in the construction and combination of instruments, have had the opportunity of putting the inferences deduced from Newton's views to the test of experiment, and all, without exception, agree in accepting them. On the other side is a man whose remarkable mental endowments, and whose singular talent for seeing through whatever obscures reality, we

have had occasion to recognise, not only in poetry, but also in the descriptive parts of the natural sciences; and this man assures us with the utmost zeal that the physicists are wrong: he is so convinced of the correctness of his own view, that he cannot explain the contradiction except by assuming narrowness or malice on their part, and finally declares that he cannot help looking upon his own achievement in the theory of colour as far more valuable than anything he has accomplished in poetry.[1]

So flat a contradiction leads us to suspect that there must be behind some deeper antagonism of principle, some difference of organisation between his mind and theirs, to prevent them from understanding each other. I will try to indicate in the following pages what I conceive to be the grounds of this antagonism.

Goethe, though he exercised his powers in many spheres of intellectual activity, is nevertheless, *par excellence*, a poet. Now in poetry, as in every other art, the essential thing is to make the material of the art, be it words, or music, or colour, the direct vehicle of an idea. In a perfect work of art, the idea must be present and dominate the whole, almost unknown to the poet himself, not as the result of a long intellectual process, but as inspired by a direct intuition of the inner eye, or by an outburst of excited feeling.

An idea thus embodied in a work of art, and dressed in the garb of reality, does indeed make a vivid impression by appealing directly to the senses, but loses, of course, that universality and that intelligibility which it would have had if presented in the form of an abstract notion. The poet, feeling how the charm of his works is involved in an intellectual process of this type, seeks to apply it to other materials. Instead of trying to arrange the phenomena of nature under definite conceptions, in-

[1] See Eckermann's *Conversations*.

dependent of intuition, he sits down to contemplate them as he would a work of art, complete in itself, and certain to yield up its central idea, sooner or later, to a sufficiently susceptible student. Accordingly, when he sees the skull on the Lido, which suggests to him the vertebral theory of the cranium, he remarks that it serves to revive his old belief, already confirmed by experience, that Nature has no secrets from the attentive observer. So again in his first conversation with Schiller on the 'Metamorphosis of Plants.' To Schiller, as a follower of Kant, the idea is the goal, ever to be sought, but ever unattainable, and therefore never to be exhibited as realised in a phenomenon. Goethe, on the other hand, as a genuine poet, conceives that he finds in the phenomenon the direct expression of the idea. He himself tells us that nothing brought out more sharply the separation between himself and Schiller. This, too, is the secret of his affinity with the natural philosophy of Schelling and Hegel, which likewise proceeds from the assumption that Nature shows us by direct intuition the several steps by which a conception is developed. Hence, too, the ardour with which Hegel and his school defended Goethe's scientific views. Moreover this view of Nature accounts for the war which Goethe continued to wage against complicated experimental researches. Just as a genuine work of art cannot bear retouching by a strange hand, so he would have us believe Nature resists the interference of the experimenter who tortures her and disturbs her; and in revenge, misleads the impertinent kill-joy by a distorted image of herself.

Accordingly, in his attack upon Newton he often sneers at spectra, tortured through a number of narrow slits and glasses, and commends the experiments that can be made in the open air under a bright sun, not merely as particularly easy and particularly enchanting, but also

as particularly convincing! The poetic turn of mind is very marked even in his morphological researches. If we only examine what has really been accomplished by the help of the ideas which he contributed to science, we shall be struck by the very singular relation which they bear to it. No one will refuse to be convinced if you lay before him the series of transformations by which a leaf passes into a stamen, an arm into a fin or a wing, a vertebra into the occipital bone. The idea that all the parts of a flower are modified leaves, reveals a connecting law, which surprises us into acquiescence. But now try and define the leaf-like organ, determine its essential characteristics, so as to include all the forms that we have named. You will find yourself in a difficulty, for all distinctive marks vanish, and you have nothing left, except that a leaf in the wider sense of the term is a lateral appendage of the axis of a plant. Try then to express the proposition 'the parts of the flower are modified leaves' in the language of scientific definition, and it reads, 'the parts of the flower are lateral appendages of the axis.' To see this does not require a Goethe. So again it has been objected, and not unjustly, to the vertebral theory, that it must extend the notion of a vertebra so much that nothing is left but the bare fact —a vertebra is a bone. We are equally perplexed if we try to express in clear scientific language what we mean by saying that such and such a part of one animal corresponds to such and such a part of another. We do not mean that their physiological use is the same, for the same piece which in a bird serves as the lower jaw, becomes in mammals a tiny tympanal bone. Nor would the shape, the position, or the connection of the part in question with other parts, serve to identify it in all cases. But yet it has been found possible in most cases, by following the intermediate steps, to determine with toler-

able certainty which parts correspond to each other. Goethe himself said this very clearly: he says, in speaking of the vertebral theory of the skull, 'Such an *aperçu*, such an intuition, conception, representation, notion, idea, or whatever you choose to call it, always retains something esoteric and indefinable, struggle as you will against it; as a general principle, it may be enunciated, but cannot be proved; in detail it may be exhibited, but can never be put in a cut and dry form.' And so, or nearly so, the problem stands to this day. The difference may be brought out still more clearly if we consider how physiology, which investigates the relations of vital processes as cause and effect, would have to treat this idea of a common type of animal structure. The science might ask, Is it, on the one hand, a correct view, that during the geological periods that have passed over the earth, one species has been developed from another, so that, for example, the breast-fin of the fish has gradually changed into an arm or a wing? Or again, shall we say that the different species of animals were created equally perfect—that the points of resemblance between them are to be ascribed to the fact, that in all vertebrate animals the first steps in development from the egg can only be effected by Nature in one way, almost identical in all cases, and that the later analogies of structure are determined by these features, common to all embryos? Probably the majority of observers incline to the latter view,[1] for the agreement between the embryos of different vertebrate animals, in the earlier stages, is very striking. Thus even young mammals have occasionally rudimentary gills on the side of the neck, like fishes. It seems, in fact, that what are in the mature animals corresponding parts, originate in the same way during the process of development, so the scientific men have

[1] This was written before the appearance of Darwin's *Origin of Species*.

lately begun to make use of embryology as a sort of check on the theoretical views of comparative anatomy. It is evident that by the application of the physiological views just suggested, the idea of a common type would acquire definiteness and meaning as a distinct scientific conception. Goethe did much: he saw by a happy intuition that there was a law, and he followed up the indications of it with great shrewdness. But what law it was, he did not see; nor did he even try to find it out. That was not in his line. Moreover, even in the present condition of science, a definite view on the question is impossible; the very form in which it should be proposed is scarcely yet settled. And therefore we readily admit that in this department Goethe did all that was possible at the time when he lived. I said just now that he treated nature like a work of art. In his studies on morphology, he reminds one of a spectator at a play, with strong artistic sympathies. His delicate instinct makes him feel how all the details fall into their places, and work harmoniously together, and how some common purpose governs the whole; and yet, while this exquisite order and symmetry give him intense pleasure, he cannot formulate the dominant idea. That is reserved for the scientific critic of the drama, while the artistic spectator feels perhaps, as Goethe did in the presence of natural phenomena, an antipathy to such dissection, fearing, though without reason, that his pleasure may be spoilt by it.

Goethe's point of view in the Theory of Colour is much the same. We have seen that he rebels against the physical theory just at the point where it gives complete and consistent explanations from principles once accepted. Evidently it is not the insufficiency of the theory to explain individual cases that is a stumbling-block to him. He takes offence at the assumption made for the

sake of explaining the phenomena, which seem to him so absurd, that he looks upon the interpretation as no interpretation at all. Above all, the idea that white light could be composed of coloured light seems to have been quite inconceivable to him; at the very beginning of the controversy, he rails at the disgusting Newtonian white of the natural philosophers, an expression which seems to show that this was the assumption that most annoyed him.

Again, in his later attacks on Newton, which were not published till after his Theory of Colour was completed, he rather strives to show that Newton's facts might be explained on his own hypothesis, and that therefore Newton's hypothesis was not fully proved, than attempts to prove that hypothesis inconsistent with itself or with the facts. Nay, he seems to consider the obviousness of his own hypothesis so overwhelming, that it need only be brought forward to upset Newton's entirely. There are only a few passages where he disputes the experiments described by Newton. Some of them, apparently, he could not succeed in refuting, because the result is not equally easy to observe in all positions of the lenses used, and because he was unacquainted with the geometrical relations by which the most favourable positions of them are determined. In other experiments on the separation of simple coloured light by means of prisms alone, Goethe's objections are not quite groundless, inasmuch as the isolation of single colours cannot by this means be so effectually carried out, that after refraction through another prism there are no traces of other tints at the edges. A complete isolation of light of one colour can only be effected by very carefully arranged apparatus, consisting of combined prisms and lenses, a set of experiments which Goethe postponed to a supplement, and finally left unnoticed. When he complains of the complication of these contrivances, we need only think of the laborious

and roundabout methods which chemists must often adopt to obtain certain elementary bodies in a pure form; and we need not be surprised to find that it is impossible to solve a similar problem in the case of light in the open air in a garden, and with a single prism in one's hand.[1] Goethe must, consistently with his theory, deny *in toto* the possibility of isolating pure light of one colour. Whether he ever experimented with the proper apparatus to solve the problem remains doubtful, as the supplement in which he promised to detail these experiments was never published.

To give some idea of the passionate way in which Goethe, usually so temperate and even courtier-like, attacks Newton, I quote from a few pages of the controversial part of his work the following expressions, which he applies to the propositions of this consummate thinker in physical and astronomical science—'incredibly impudent;' 'mere twaddle;' 'ludicrous explanation;' 'admirable for school-children in a go-cart;' 'but I see nothing will do but lying, and plenty of it.'[2]

Thus, in the theory of colour, Goethe remains faithful to his principle, that Nature must reveal her secrets of her own free will; that she is but the transparent representation of the ideal world. Accordingly, he demands as a preliminary to the investigation of physical phenomena, that the observed facts shall be so arranged that one explains the other, and that thus we may attain an insight

[1] I venture to add that I am acquainted with the impossibility of decomposing or changing simple coloured light, the two principles which form the basis of Newton's theory, not merely by hearsay, but from actual observation, having been under the necessity in one of my own researches of obtaining light of one colour in a state of the greatest possible purity. (See Poggendorff's *Annalen*, vol. lxxxvi. p. 501, on Sir D. Brewster's *New Analysis of Sunlight*.)

[2] Something parallel to this extraordinary proceeding of Goethe's may be found in Hobbes's attack on Wallis.—Tr.

into their connection without ever having to trust to anything but our senses. This demand of his looks most attractive, but is essentially wrong in principle. For a natural phenomenon is not considered in physical science to be fully explained until you have traced it back to the ultimate forces which are concerned in its production and its maintenance. Now, as we can never become cognizant of forces *qua* forces, but only of their effects, we are compelled in every explanation of natural phenomena to leave the sphere of sense, and to pass to things which are not objects of sense, and are defined only by abstract conceptions. When we find a stove warm, and then observe that a fire is burning in it, we say, though somewhat inaccurately, that the former sensation is explained by the latter. But in reality this is equivalent to saying, we are always accustomed to find heat where fire is burning; now, a fire is burning in the stove, therefore we shall find heat there. Accordingly we bring our single fact under a more general, better known fact, rest satisfied with it, and call it falsely an explanation. Evidently, however, the generality of the observation does not necessarily imply an insight into causes; such an insight is only obtained when we can make out what forces are at work in the fire, and how the effects depend upon them.

But this step into the region of abstract conceptions, which must necessarily be taken, if we wish to penetrate to the causes of phenomena, scares the poet away. In writing a poem he has been accustomed to look, as it were, right into the subject, and to reproduce his intuition without formulating any of the steps that led him to it. And his success is proportionate to the vividness of the intuition. Such is the fashion in which he would have Nature attacked. But the natural philosopher insists on transporting him into a world of invisible atoms and movements, of attractive and repulsive forces, whose

intricate actions and reactions, though governed by strict laws, can scarcely be taken in at a glance. To him the impressions of sense are not an irrefragable authority; he examines what claim they have to be trusted; he asks whether things which they pronounce alike are really alike, and whether things which they pronounce different are really different; and often finds that he must answer, no! The result of such examination, as at present understood, is that the organs of sense do indeed give us information about external effects produced on them, but convey those effects to our consciousness in a totally different form, so that the character of a sensuous perception depends not so much on the properties of the object perceived as on those of the organ by which we receive the information. All that the optic nerve conveys to us, it conveys under the form of a sensation of light, whether it be the rays of the sun, or a blow in the eye, or an electric current passing through it. Again, the auditory nerve translates everything into phenomena of sound, the nerves of the skin into sensations of temperature or touch. The same electric current whose existence is indicated by the optic nerve as a flash of light, or by the organ of taste as an acid flavour, excites in the nerves of the skin the sensation of burning. The same ray of sunshine, which is called light when it falls on the eye, we call heat when it falls on the skin. But on the other hand, in spite of their different effects upon our organisation, the daylight which enters through our windows, and the heat radiated by an iron stove, do not in reality differ more or less from each other than the red and blue constituents of light. In fact, just as in the Undulatory Theory, the red rays are distinguished from the blue rays only by their longer period of vibration, and their smaller refrangibility, so the dark heat rays of the stove have a still longer period and still smaller re-

frangibility than the red rays of light, but are in every other respect exactly similar to them. All these rays, whether luminous or non-luminous, have heating properties, but only a certain number of them, to which for that reason we give the name of light, can penetrate through the transparent part of the eye to the optic nerve, and excite a sensation of light. Perhaps the relation between our senses and the external world may be best enunciated as follows: our sensations are for us only *symbols* of the objects of the external world, and correspond to them only in some such way as written characters or articulate words to the things they denote. They give us, it is true, information respecting the properties of things without us, but no better information than we give a blind man about colour by verbal descriptions.

We see that science has arrived at an estimate of the senses very different from that which was present to the poet's mind. And Newton's assertion that white was composed of all the colours of the spectrum was the first germ of the scientific view which has subsequently been developed. For at that time there were none of those galvanic observations which paved the way to a knowledge of the functions of the nerves in the production of sensations. Natural philosophers asserted that white, to the eye the simplest and purest of all our sensations of colour, was compounded of less pure and complex materials. It seems to have flashed upon the poet's mind that all his principles were unsettled by the results of this assertion, and that is why the hypothesis seems to him so unthinkable, so ineffably absurd. We must look upon his theory of colour as a forlorn hope, as a desperate attempt to rescue from the attacks of science the belief in the direct truth of our sensations. And this will account for the enthusiasm with which he strives to elaborate and to defend his theory, for the passionate irritability with

which he attacks his opponent, for the overweening importance which he attaches to these researches in comparison with his other achievements, and for his inaccessibility to conviction or compromise.

If we now turn to Goethe's own theories on the subject, we must, on the grounds above stated, expect to find that he cannot, without being untrue to his own principle, give us anything deserving to be called a scientific explanation of the phenomena, and that is exactly what happens. He starts with the proposition that all colours are darker than white, that they have something of shade in them (on the physical theory, white compounded of all colours must necessarily be brighter than any of its constituents). The direct mixture of dark and light, of black and white, gives grey; the colours must therefore owe their existence to some form of the co-operation of light and shade. Goethe imagines he has discovered it in the phenomena presented by slightly opaque or hazy media. Such media usually look blue when the light falls on them, and they are seen in front of a dark object, but yellow when a bright object is looked at through them. Thus in the day time the air looks blue against the dark background of the sky, and the sun, when viewed, as is the case at sunset, through a thick and hazy stratum of air, appears yellow. The physical explanation of this phenomenon, which, however, is not exhibited by all such media, as, for instance, by plates of unpolished glass, would lead us too far from the subject. According to Goethe, the semi-opaque medium imparts to the light something corporeal, something of the nature of shade, such as is requisite, he would say, for the formation of colour. This conception alone is enough to perplex anyone who looks upon it as a physical explanation. Does he mean to say that material particles mingle with the light and fly away with

it? But this is Goethe's fundamental experiment, this is the typical phenomenon under which he tries to reduce all the phenomena of colour, especially those connected with the prismatic spectrum. He looks upon all transparent bodies as slightly hazy, and assumes that the prism imparts to the image which it shows to an observer something of its own opacity. Here, again, it is hard to get a definite conception of what is meant. Goethe seems to have thought that a prism never gives perfectly defined images, but only indistinct, half-obliterated ones, for he puts them all in the same class with the double images which are exhibited by parallel plates of glass and by Iceland spar. The images formed by a prism are, it is true, indistinct in compound light, but they are perfectly defined when simple light is used. If you examine, he says, a bright surface on a dark ground through a prism, the image is displaced and blurred by the prism. The anterior edge is pushed forward over the dark background, and consequently a hazy light on a dark ground appears blue, while the other edge is covered by the image of the black surface which comes after it, and, consequently, being a light image behind a hazy dark colour, appears yellowish-red. But why the anterior edge appears in front of the ground, the posterior edge behind it, and not *vice versa*, he does not explain. Let us analyse this explanation, and try to grasp clearly the conception of an optical image. When I see a bright object reflected in a mirror, the reason is that the light which proceeds from it is thrown back exactly as if it came from an object of the same kind behind the mirror. The eye of the observer receives the impression accordingly, and therefore he imagines he really sees the object. Everyone knows there is nothing real behind the mirror to correspond to the image—that no light can penetrate thither, but that what is called the image is

simply a geometrical point, in which the reflected rays, if produced backwards, would intersect. And, accordingly, no one expects the image to produce any real effect behind the mirror. In the same way the prism shows us images of objects which occupy a different position from the objects themselves; that is to say, the light which an object sends to the prism is refracted by it, so that it appears to come from an object lying to one side, called the image. This image, again, is not real; it is, as in the case of reflection, the geometrical point in which the refracted rays intersect when produced backwards. And yet, according to Goethe, this image is to produce real effects by its displacement; the displaced patch of light makes, he says, the dark space behind it appear blue, just as an imperfectly transparent body would, and so again the displaced dark patch makes the bright space behind appear reddish-yellow. That Goethe really treats the image as an actual object in the place it appears to occupy is obvious enough, especially as he is compelled to assume, in the course of his explanation, that the blue and red edges of the bright space are respectively before and behind the dark image which, like it, is displaced by the prism. He does, in fact, remain loyal to the appearance presented to the senses, and treats a geometrical locus as if it were a material object. Again, he does not scruple at one time to make red and blue destroy each other, as, for example, in the blue edge of a red surface seen through the prism, and at another to construct out of them a beautiful purple, as when the blue and red edges of two neighbouring white surfaces meet in a black ground. And when he comes to Newton's more complicated experiments, he is driven to still more marvellous expedients. As long as you treat his explanations as a pictorial way of representing the physical processes, you may acquiesce in them, and even frequently

find them vivid and characteristic, but as physical elucidations of the phenomena they are absolutely irrational.

In conclusion, it must be obvious to everyone that the theoretical part of the Theory of Colour is not natural philosophy at all; at the same time we can, to a certain extent, see that the poet wanted to introduce a totally different method into the study of Nature, and more or less understand how he came to do so. Poetry is concerned solely with the 'beautiful show' which makes it possible to contemplate the ideal; how that show is produced is a matter of indifference. Even Nature is, in the poet's eyes, but the sensible expression of the spiritual. The natural philosopher, on the other hand, tries to discover the levers, the cords, and the pulleys, which work behind the scenes, and shift them. Of course the sight of the machinery spoils the beautiful show, and therefore the poet would gladly talk it out of existence, and ignoring cords and pulleys as the chimeras of a pedant's brain, he would have us believe that the scenes shift themselves, or are governed by the idea of the drama. And it is just characteristic of Goethe, that he, and he alone among poets, must needs break a lance with natural philosophers. Other poets are either so entirely carried away by the fire of their enthusiasm that they do not trouble themselves about the disturbing influences of the outer world, or else they rejoice in the triumphs of mind over matter, even on that unpropitious battlefield. But Goethe, whom no intensity of subjective feeling could blind to the realities around him, cannot rest satisfied until he has stamped reality itself with the image and superscription of poetry. This constitutes the peculiar beauty of his poetry, and at the same time fully accounts for his resolute hostility to the machinery that every moment threatens to disturb his poetic repose, and for his determination to attack the enemy in his own camp.

But we cannot triumph over the machinery of matter by ignoring it; we can triumph over it only by subordinating it to the aims of our moral intelligence. We must familiarise ourselves with its levers and pulleys, fatal though it be to poetic contemplation, in order to be able to govern them after our own will, and therein lies the complete justification of physical investigation, and its vast importance for the advance of human civilisation.

From what I have said it will be apparent that Goethe did follow the same line of thought in all his contributions to science, but that the problems he encountered were of diametrically opposite characters. And, perhaps, when it is understood how the self-same characteristic of his intellect, which in one branch of science won for him immortal renown, entailed upon him egregious failure in the other, it will tend to dissipate, in the minds of many worshippers of the great poet, a lingering prejudice against natural philosophers, whom they suspect of being blinded by narrow professional pride to the loftiest inspirations of genius.

ON THE

PHYSIOLOGICAL CAUSES OF HARMONY IN MUSIC.

A LECTURE DELIVERED IN BONN DURING THE WINTER OF 1857.

LADIES AND GENTLEMEN,—In the native town of Beethoven, the mightiest among the heroes of harmony, no subject seemed to me better adapted for a popular audience than music itself. Following, therefore, the direction of my researches during the last few years, I will endeavour to explain to you what physics and physiology have to say regarding the most cherished art of the Rhenish land—music and musical relations. Music has hitherto withdrawn itself from scientific treatment more than any other art. Poetry, painting, and sculpture borrow at least the material for their delineations from the world of experience. They portray nature and man. Not only can their material be critically investigated in respect to its correctness and truth to nature, but scientific art-criticism, however much enthusiasts may have disputed its right to do so, has actually succeeded in making some progress in investigating the causes of that æsthetic pleasure which it is the intention of these arts to excite. In music, on the other hand, it seems at first

sight as if those were still in the right who reject all 'anatomisation of pleasurable sensations.' This art, borrowing no part of its material from the experience of our senses; not attempting to describe, and only exceptionally to imitate the outer world, necessarily withdraws from scientific consideration the chief points of attack which other arts present, and hence seems to be as incomprehensible and wonderful as it is certainly powerful in its effects. We are, therefore, obliged, and we purpose, to confine ourselves, in the first place, to a consideration of the material of the art, musical sounds or sensations. It always struck me as a wonderful and peculiarly interesting mystery, that in the theory of musical sounds, in the physical and technical foundations of music, which above all other arts seems in its action on the mind as the most immaterial, evanescent, and tender creator of incalculable and indescribable states of consciousness, that here in especial the science of purest and strictest thought—mathematics—should prove preeminently fertile. Thorough bass is a kind of applied mathematics. In considering musical intervals, divisions of time, and so forth, numerical fractions, and sometimes even logarithms, play a prominent part. Mathematics and music! the most glaring possible opposites of human thought! and yet connected, mutually sustained! It is as if they would demonstrate the hidden consensus of all the actions of our mind, which in the revelations of genius makes us forefeel unconscious utterances of a mysteriously active intelligence.

When I considered physical acoustics from a physiological point of view, and thus more closely followed up the part which the ear plays in the perception of musical sounds, much became clear of which the connection had not been previously evident. I will attempt to inspire you with some of the interest which these questions have

awakened in my own mind, by endeavouring to exhibit a few of the results of physical and physiological acoustics.

The short space of time at my disposal obliges me to confine my attention to one particular point; but I shall select the most important of all, which will best show you the significance and results of scientific investigation in this field; I mean the foundation of concord. It is an acknowledged fact that the numbers of the vibrations of concordant tones bear to each other ratios expressible by small whole numbers. But why? What have the ratios of small whole numbers to do with concord? This is an old riddle, propounded by Pythagoras, and hitherto unsolved. Let us see whether the means at the command of modern science will furnish the answer.

First of all, what is a musical tone? Common experience teaches us that all sounding bodies are in a state of vibration. This vibration can be seen and felt; and in the case of loud sounds we feel the trembling of the air even without touching the sounding bodies. Physical science has ascertained that any series of impulses which produce a vibration of the air will, if repeated with sufficient rapidity, generate sound.

This sound becomes a *musical* tone, when such rapid impulses recur with perfect regularity and in precisely equal times. Irregular agitation of the air generates only noise. The *pitch* of a musical tone depends on the number of impulses which take place in a given time; the more there are in the same time the higher or sharper is the tone. And, as before remarked, there is found to be a close relationship between the well-known harmonious musical intervals and the number of the vibrations of the air. If twice as many vibrations are performed in the same time for one tone as for another, the first is the octave above the second. If the numbers of vibrations

in the same time are as 2 to 3, the two tones form a fifth; if they are as 4 to 5, the two tones form a major third.

If you observe that the numbers of the vibrations which generate the tones of the major chord C E G c are in the ratio of the numbers 4 : 5 : 6 : 8, you can deduce from these all other relations of musical tones, by imagining a new major chord, having the same relations of the numbers of vibrations, to be formed upon each of the above-named tones. The numbers of vibrations within the limits of audible tones which would be obtained by executing the calculation thus indicated, are extraordinarily different. Since the octave above any tone has twice as many vibrations as the tone itself, the second octave above will have four times, the third has eight times as many. Our modern pianofortes have seven octaves. Their highest tones, therefore, perform 128 vibrations in the time that their lowest tone makes one single vibration.

The deepest C_1 which our pianos usually possess, answers to the sixteen-foot open pipe of the organ—musicians call it the 'contra-C'—and makes thirty-three vibrations in one second of time. This is very nearly the limit of audibility. You will have observed that these tones have a dull, bad quality of sound on the piano, and that it is difficult to determine their pitch and the accuracy of their tuning. On the organ the contra-C is somewhat more powerful than on the piano, but even here some uncertainty is felt in judging of its pitch. On larger organs there is a whole octave of tones below the contra-C, reaching to the next lower C, with $16\frac{1}{2}$ vibrations in a second. But the ear can scarcely separate these tones from an obscure drone; and the deeper they are the more plainly can it distinguish the separate impulses of the air to which they are due. Hence they are used solely in conjunction with the next higher octaves, to strengthen their notes, and produce an impression of greater depth.

With the exception of the organ, all musical instruments, however diverse the methods in which their sounds are produced, have their limit of depth at about the same point in the scale as the piano; not because it would be impossible to produce slower impulses of the air of sufficient power, but because the *ear* refuses its office, and hears slower impulses separately, without gathering them up into single tones.

The often repeated assertion of the French physicist Savart, that he heard tones of eight vibrations in a second, upon a peculiarly constructed instrument, seems due to an error.

Ascending the scale from the contra-C, pianofortes usually have a compass of seven octaves, up to the so-called five-accented *c*, which has 4,224 vibrations in a second. Among orchestral instruments it is only the piccolo flute which can reach as high, and this will give even one tone higher. The violin usually mounts no higher than the *e* below, which has 2,640 vibrations—of course we except the gymnastics of heaven-scaling *virtuosi*, who are ever striving to excruciate their audience by some new impossibility. Such performers may aspire to three whole octaves lying above the five-accented *c*, and very painful to the ear, for their existence has been established by Despretz, who, by exciting small tuning-forks with a violin bow, obtained and heard the eight-accented *c*, having 32,770 vibrations in a second. Here the sensation of tone seemed to have reached its upper limit, and the intervals were really undistinguishable in the later octaves.

The musical pitch of a tone depends entirely on the number of vibrations of the air in a second, and not at all upon the mode in which they are produced. It is quite indifferent whether they are generated by the vibrating strings of a piano or violin, the vocal chords of

the human larynx, the metal tongues of the harmonium, the reeds of the clarionet, oboe and bassoon, the trembling lips of the trumpeter, or the air cut by a sharp edge in organ pipes and flutes.

A tone of the same number of vibrations has always the same pitch, by whichever one of these instruments it is produced. That which distinguishes the note A of a piano for example, from the equally high A of the violin, flute, clarionet, or trumpet, is called the *quality of the tone*, and to this we shall have to recur presently.

As an interesting example of these assertions, I beg to show you a peculiar physical instrument for producing musical tones, called the *siren*, Fig. 1, which is especially adapted to establish the properties resulting from the ratios of the numbers of vibrations.

In order to produce tones upon this instrument, the portvents g_0 and g_1 are connected by means of flexible tubes with a bellows. The air enters into round brass boxes, a_0 and a_1, and escapes by the perforated covers of these boxes at c_0 and c_1. But the holes for the escape of air are not perfectly free. Immediately before the covers of both boxes there are two other perforated discs, fastened to a perpendicular axis k, which turns with great readiness. In the figure, only the perforated disc can be seen at c_0, and immediately below it is the similarly perforated cover of the box. In the upper box, c_1, only the edge of the disc is visible. If then the holes of the disc are precisely opposite to those of the cover, the air can escape freely. But if the disc is made to revolve, so that some of its unperforated portions stand before the holes of the box, the air cannot escape at all. On turning the disc rapidly, the vent-holes of the box are alternately opened and closed. During the opening, air escapes; during the closure, no air can pass. Hence the continuous stream of air from the bellows is converted into a series of discontinuous puffs, which, when they follow one another with sufficient rapidity, gather themselves together into a tone.

Each of the revolving discs of this instrument (which is more complicated in its construction than any one of the kind hitherto made, and hence admits of a much greater number of combina-

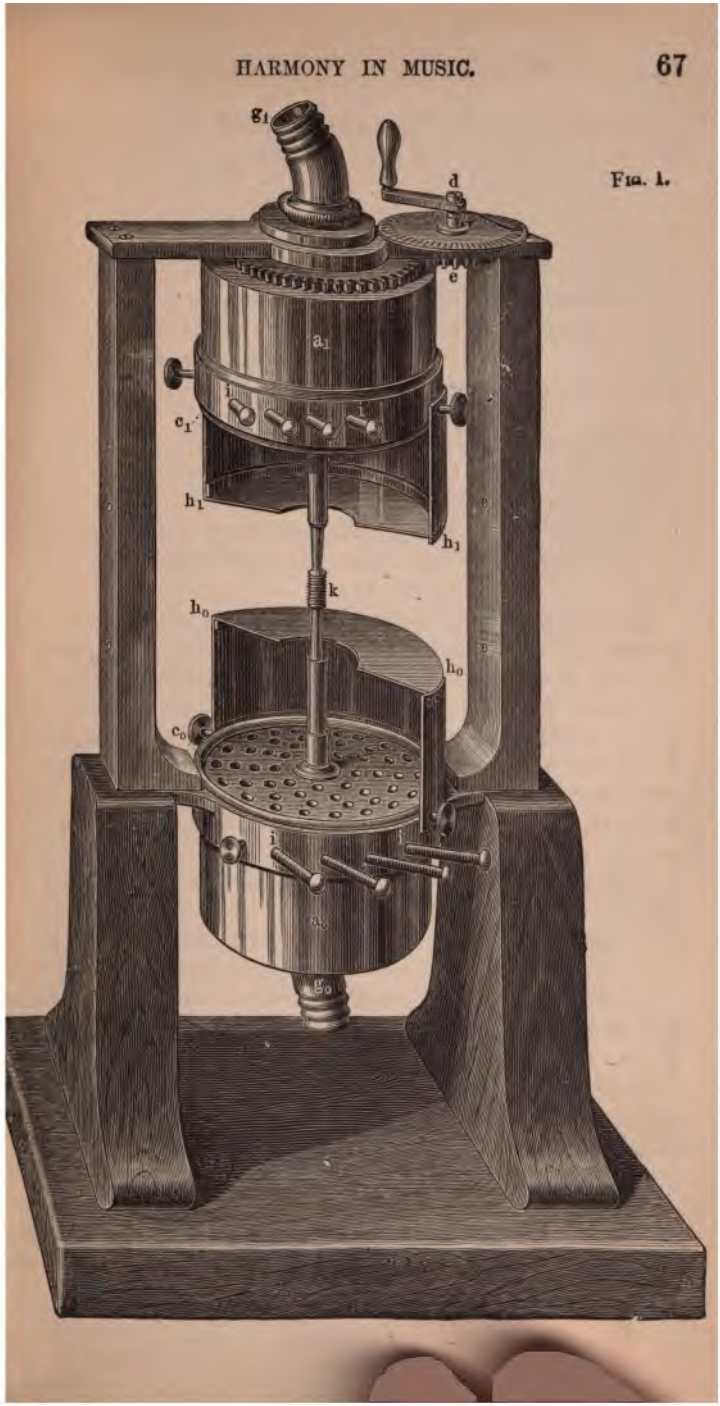


FIG. 1.

tions of tone) has four concentric circles of holes, the lower set having 8, 10, 12, 18, and the upper set 9, 12, 15, and 16, holes respectively. The series of holes in the covers of the boxes are precisely the same as those in the discs, but under each of them lies a perforated ring, which can be so arranged, by means of the stops i i i i, that the corresponding holes of the cover can either communicate freely with the inside of the box, or are entirely cut off from it. We are thus enabled to use any one of the eight series of holes singly, or combined two and two, or three and three together, in any arbitrary manner.

The round boxes, h_0 h_0 and h_1 h_1, of which halves only are drawn in the figure, serve by their resonance to soften the harshness of the tone.

The holes in the boxes and discs are cut obliquely, so that when the air enters the boxes through one or more of the series of holes, the wind itself drives the discs round with a perpetually increasing velocity.

On beginning to blow the instrument, we first hear separate impulses of the air, escaping as puffs, as often as the holes of the disc pass in front of those of the box. These puffs of air follow one another more and more quickly, as the velocity of the revolving discs increases, just like the puffs of steam of a locomotive on beginning to move with the train. They next produce a whirring and whizzing, which constantly becomes more rapid. At last we hear a dull drone, which, as the velocity further increases, gradually gains in pitch and strength.

Suppose that the discs have been brought to a velocity of 33 revolutions in a second, and that the series with 8 holes has been opened. At each revolution of the disc all these 8 holes will pass before each separate hole of the cover. Hence there will be 8 puffs for each revolution of the disc, or 8 times 33, that is, 264 puffs in a second. This gives us the once-accented *c* of our musical scale, [that is 'middle *c*,' written on the leger line between the bass and treble staves.] But on opening the series of 16 holes instead, we have twice as many, or 16 times 33, that is, 528 vibrations in a second. We hear exactly the octave above the first *c'*, that is the twice-accented *c''*, [or *c* on the third space of the treble staff.] By opening both the series of 8 and 16 holes at once, we have both *c'* and *c''* at once, and can convince ourselves that we have the absolutely pure concord of the

octave. By taking 8 and 12 holes, which give numbers of vibrations in the ratio of 2 to 3, we have the concord of a perfect fifth. Similarly 12 and 16 or 9 and 12 give fourths, 12 and 15 give a major third, and so on.

The upper box is furnished with a contrivance for slightly sharpening or flattening the tones which it produces. This box is movable upon an axis, and connected with a toothed wheel, which is worked by the driver attached to the handle d. By turning the handle slowly while one of the series of holes in the upper box is in use, the tone will be sharper or flatter, according as the box moves in the opposite direction to the disc, or in the same direction as the disc. When the motion is in the opposite direction, the holes meet those of the disc a little sooner than they otherwise would, the time of vibration of the tone is shortened, and the tone becomes sharper. The contrary ensues in the other case.

Now, on blowing through 8 holes below and 16 above, we have a perfect octave, as long as the upper box is still; but when it is in motion, the pitch of the upper tone is slightly altered, and the octave becomes false.

On blowing through 12 holes above and 18 below, the result is a perfect fifth as long as the upper box is at rest, but if it moves the concord is perceptibly injured.

These experiments with the siren show us, therefore:—

1. That a series of puffs following one another with sufficient rapidity, produce a musical tone.

2. That the more rapidly they follow one another, the sharper is the tone.

3. That when the ratio of the number of vibrations is exactly 1 to 2, the result is a perfect octave; when it is 2 to 3, a perfect fifth; when it is 3 to 4, a pure fourth, and so on. The slightest alteration in these ratios destroys the purity of the concord.

You will perceive, from what has been hitherto adduced, that the human ear is affected by vibrations of the air, within certain degrees of rapidity—viz. from about 20 to about 32,000 in a second—and that the sensation of musical tone arises from this affection.

That the sensation thus excited is a sensation of musical tone, does not depend in any way upon the peculiar manner in which the air is agitated, but solely on the peculiar powers of sensation possessed by our ears and auditory nerves. I remarked, a little while ago, that when the tones are loud the agitation of the air is perceptible to the skin. In this way deaf mutes can perceive the motion of the air, which we call sound. But they do not hear, that is, they have no sensation of tone in the ear. They feel the motion by the nerves of the skin, producing that peculiar description of sensation called whirring. The limits of the rapidity of vibration within which the ear feels an agitation of the air to be sound, depend also wholly upon the peculiar constitution of the ear.

When the siren is turned slowly, and hence the puffs of air succeed each other slowly, you hear no musical sound. By the continually increasing rapidity of its revolution, no essential change is produced in the kind of vibration of the air. Nothing new happens externally to the ear. The only new result is the sensation experienced by the ear, which then for the first time begins to be affected by the agitation of the air. Hence the more rapid vibrations receive a new name, and are called Sound. If you admire paradoxes, you may say that aerial vibrations do not become sound until they fall upon a hearing ear.

I must now describe the propagation of sound through the atmosphere. The motion of a mass of air through which a tone passes, belongs to the so-called wave motions —a class of motions of great importance in physics. Light, as well as sound, is one of these motions.

The name is derived from the analogy of waves on the surface of water, and these will best illustrate the peculiarity of this description of motion.

When a point in a surface of still water is agitated—as

by throwing in a stone—the motion thus caused is propagated in the form of waves, which spread in rings over the surface of the water. The circles of waves continue to increase even after rest has been restored at the point first affected. At the same time the waves become continually lower, the further they are removed from the centre of motion, and gradually disappear. On each wave-ring we distinguish ridges or crests, and hollows or troughs.

Crest and trough together form a wave, and we measure its length from one crest to the next.

While the wave passes over the surface of the fluid, the particles of the water which form it do not move on with it. This is easily seen, by floating a chip of straw on the water. When the waves reach the chip, they raise or depress it, but when they have passed over it, the position of the chip is not perceptibly changed.

Now a light floating chip has no motion different from that of the adjacent particles of water. Hence we conclude that these particles do not follow the wave, but, after some pitching up and down, remain in their original position. That which really advances as a wave is, consequently, not the particles of water themselves, but only a superficial form, which continues to be built up by fresh particles of water. The paths of the separate particles of water are more nearly vertical circles, in which they revolve with a tolerably uniform velocity, as long as the waves pass over them.

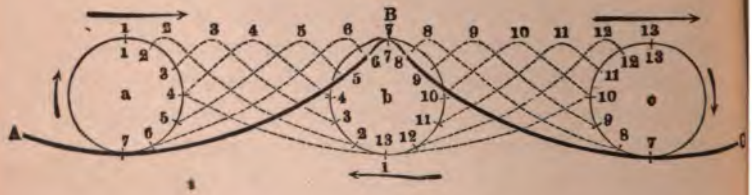
In Fig. 2 the dark wave-line, A B C, represents a section of the surface of the water, over which waves are running in the direction of the arrows above a and c. The three circles, a, b, and c, represent the paths of particular particles of water at the surface of the wave. The particle which revolves in the circle b, is supposed at the time that the surface of the water presents the form A B C, to be at its highest point B, and the particles re-

volving in the circles a and c to be simultaneously in their lowest positions.

The respective particles of water revolve in these circles in the direction marked by the arrows. The dotted curves represent other positions of the passing waves, at equal intervals of time, partly before the assumption of the A B C position (as for the crests between a and b), and partly after the same (for the crests between b and c). The positions of the crests are marked with figures. The same figures in the three circles, show where the respective revolving particle would be, at the moment the wave assumed the corresponding form. It will be noticed that the particles advance by equal arcs of the circles, as the crest of the wave advances by equal distances parallel to the water level.

In the circle b it will be further seen, that the particle of water in its positions 1, 2, 3, hastens to meet the approaching

FIG. 2.



wave-crests, 1, 2, 3, rises on its left hand side, is then carried on by the crest from 4 to 7 in the direction of its advance, afterwards halts behind it, sinks down again on the right side, and finally reaches its original position at 13. (In the Lecture itself, Fig. 2 was replaced by a working model, in which the movable particles, connected by threads, really revolved in circles, while connecting elastic threads represented the surface of the water.)

All particles at the surface of the water, as you see by this drawing, describe equal circles. The particles of water at different depths move in the same way, but as the depths increase, the diameters of their circles of revolution rapidly diminish.

In this way, then, arises the appearance of a progressive motion along the surface of the water, while in reality the moving particles of water do not advance with the wave, but perpetually revolve in their small circular orbits.

To return from waves of water to waves of sound. Imagine an elastic fluid like air to replace the water, and the waves of this replaced water to be compressed by an inflexible plate laid on their surface, the fluid being prevented from escaping laterally from the pressure. Then on the waves being thus flattened out, the ridges where the fluid had been heaped up will produce much greater density than the hollows, from which the fluid had been removed to form the ridges. Hence the ridges are replaced by condensed strata of air, and the hollows by rarefied strata. Now further imagine that these compressed waves are propagated by the same law as before, and that also the vertical circular orbits of the several particles of water are compressed into horizontal straight lines. Then the waves of sound will retain the peculiarity of having the particles of air only oscillating backwards and forwards in a straight line, while the wave itself remains merely a progressive form of motion, continually composed of fresh particles of air. The immediate result then would be waves of sound spreading out horizontally from their origin.

But the expansion of waves of sound is not limited, like those of water, to a horizontal surface. They can spread out in any direction whatsoever. Suppose the circles generated by a stone thrown into the water to extend in all directions of space, and you will have the spherical waves of air by which sound is propagated.

Hence we can continue to illustrate the peculiarities of the motion of sound, by the well-known visible motions of waves of water.

The length of a wave of water, measured from crest to crest, is extremely different. A falling drop, or a breath of air, gently curls the surface of the water. The waves in the wake of a steamboat toss the swimmer or skiff severely. But the waves of a stormy ocean can find room

in their hollows for the keel of a ship of the line, and their ridges can scarcely be overlooked from the mast-head. The waves of sound present similar differences. The little curls of water with short lengths of wave correspond to high tones, the giant ocean billows to deep tones. Thus the contrabass C has a wave thirty-five feet long, its higher octave a wave of half the length, while the highest tones of a piano have waves of only three inches in length.[1]

You perceive that the pitch of the tone corresponds to the length of the wave. To this we should add that the height of the ridges, or, transferred to air, the degree of alternate condensation and rarefaction, corresponds to the loudness and intensity of the tone. But waves of the same height may have different forms. The crest of the ridge, for example, may be rounded off or pointed. Corresponding varieties also occur in waves of sound of the same pitch and loudness. The so-called *timbre* or quality of tone is what corresponds to the *form* of the waves of water. The conception of form is transferred from waves of water to waves of sound. Supposing waves of water of *different* forms to be pressed flat as before, the surface, having been levelled, will of course display no differences of form, but, in the interior of the mass of water, we shall have different distributions of pressure, and hence of density, which exactly correspond to the differences of form in the still uncompressed surface. In this sense then we can continue to speak of the form of waves of sound, and can represent it geometrically. We make the curve rise where the pressure, and hence density, increases, and fall where it diminishes—just as if we had

[1] The exact lengths of waves corresponding to certain *notes*, or symbols of tone, depend upon the standard pitch assigned to one particular note, and this differs in different countries. Hence the figures of the author have been left unreduced. They are sufficiently near to those usually adopted in England, to occasion no difficulty to the reader in these general remarks.—Tr.

a compressed fluid beneath the curve, which would expand to the height of the curve in order to regain its natural density.

Unfortunately, the form of waves of sound, on which depends the quality of the tones produced by various sounding bodies, can at present be assigned in only a very few cases.

Among the forms of waves of sound which we are able to determine with more exactness, is one of great importance, here termed the *simple* or *pure* wave-form, and represented in Fig. 3.

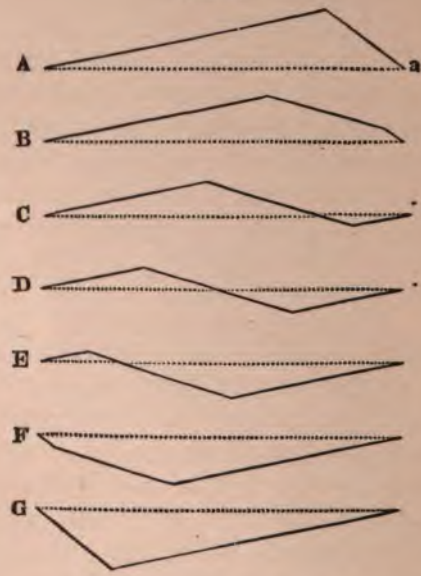
FIG. 3.

It can be seen in waves of water only when their height is small in comparison with their length, and they run over a smooth surface without external disturbance, or without any action of wind. Ridge and hollow are gently rounded off, equally broad and symmetrical, so that, if we inverted the curve, the ridges would exactly fit into the hollows, and conversely. This form of wave would be more precisely defined by saying that the particles of water describe exactly circular orbits of small diameters, with exactly uniform velocities. To this simple wave-form corresponds a peculiar species of tone, which, from reasons to be hereafter assigned, depending upon its relation to quality, we will term a *simple* tone. Such tones are produced by striking a tuning-fork, and holding it before the opening of a properly-tuned resonance tube. The tone of tuneful human voices, singing the vowel *oo* in *too*, in the middle positions of their register, appears not to differ materially from this form of wave.

We also know the laws of the motion of strings with

sufficient accuracy to assign in some cases the form of motion which they impart to the air. Thus Fig. 4 represents the forms successively assumed by a string struck, as in the German *Zither*, by a pointed style, [the *plectrum*

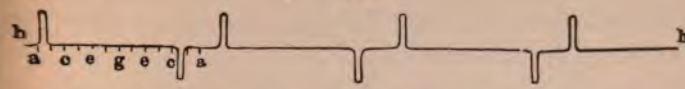
Fig. 4.



of the ancient *lyra*, or the quill of the old harpsichord, which may be easily imitated on a guitar]. A a represents the form assumed by the string at the moment of percussion. Then, at equal intervals of time, follow the forms B, C, D, E, F, G; and then in inverse order, F, E, D, C, B, A, and so on in perpetual repetition. The form of motion which such a string, by means of an attached sounding board, imparts to the surrounding air, probably corresponds to the broken line in Fig. 5, where h h indicates the position of equilibrium, and the letters a b c d e f g show the line of the wave which is produced by the action

of several forms of string marked by the corresponding capital letters in Fig. 4. It is easily seen how greatly this form of wave (which of course could not occur in

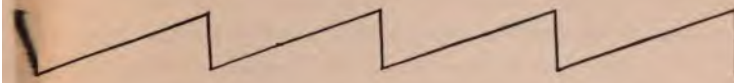
Fig. 5.



water) differs from that of Fig. 3 (independently of magnitude), as the string only imparts to the air a series of short impulses, alternately directed to opposite sides.[1]

The waves of air produced by the tone of a violin would, on the same principle, be represented by Fig. 6.

Fig. 6.



During each period of vibration the pressure increases uniformly, and at the end falls back suddenly to its minimum.

It is to such differences in the forms of the waves of sound that the variety of quality in musical tones is due. We may even carry the analogy further. The more uniformly rounded the form of wave, the softer and milder is the quality of tone. The more jerking and angular the wave-form, the more piercing the quality. Tuning-forks, with their rounded forms of wave (Fig. 3), have an extraordinarily soft quality; and the qualities of tone generated by the zither and violin resemble in harshness the angularity of their wave-forms. (Figs. 5 and 6.)

[1] It is here assumed that the sounding-board and air in contact with it immediately obey the impulse given by the end of the string without exercising a perceptible reaction on the motion of the string.

Finally, I would direct your attention to an instructive spectacle, which I have never been able to view without a certain degree of physico-scientific delight, because it displays to the bodily eye, on the surface of water, what otherwise could only be recognised by the mind's eye of the mathematical thinker in a mass of air traversed in all directions by waves of sound. I allude to the composition of many different systems of waves, as they pass over one another, each undisturbedly pursuing its own path. We can watch it from the parapet of any bridge spanning a river, but it is most complete and sublime when viewed from a cliff beside the sea. It is then rare not to see innumerable systems of waves, of various length, propagated in various directions. The longest come from the deep sea and dash against the shore. Where the boiling breakers burst shorter waves arise, and run back again towards the sea. Perhaps a bird of prey darting after a fish gives rise to a system of circular waves, which, rocking over the undulating surface, are propagated with the same regularity as on the mirror of an inland lake. And thus, from the distant horizon, where white lines of foam on the steel-blue surface betray the coming trains of wave, down to the sand beneath our feet, where the impression of their arcs remains, there is unfolded before our eyes a sublime image of immeasurable power and unceasing variety, which, as the eye at once recognises its pervading order and law, enchains and exalts without confusing the mind.

Now, just in the same way you must conceive the air of a concert-hall or ballroom traversed in every direction, and not merely on the surface, by a variegated crowd of intersecting wave-systems. From the mouths of the male singers proceed waves of six to twelve feet in length; from the lips of the songstresses dart shorter waves, from six inches long. The rustling of silken

skirts excites little curls in the air, each instrument in the orchestra emits its peculiar waves, and all these systems expand spherically from their respective centres, dart through each other, are reflected from the walls of the room, and thus rush backwards and forwards, until they succumb to the greater force of newly generated tones.

Although this spectacle is veiled from the material eye, we have another bodily organ, the ear, specially adapted to reveal it to us. This analyses the interdigitation of the waves, which in such cases would be far more confused than the intersection of the water undulations, separates the several tones which compose it, and distinguishes the voices of men and women—nay, even of individuals—the peculiar qualities of tone given out by each instrument, the rustling of the dresses, the footfalls of the walkers, and so on.

It is necessary to examine the circumstances with greater minuteness. When a bird of prey dips into the sea, rings of waves arise, which are propagated as slowly and regularly upon the moving surface as upon a surface at rest. These rings are cut into the curved surface of the waves in precisely the same way as they would have been into the still surface of a lake. The form of the external surface of the water is determined in this, as in other more complicated cases, by taking the height of each point to be the height of all the ridges of the waves which coincide at this point at one time, after deducting the sum of all similarly simultaneously coincident hollows. Such a sum of positive magnitudes (the ridges) and negative magnitudes (the hollows), where the latter have to be subtracted instead of being added, is called an *algebraical sum*. Using this term, then, we may say that *the height of every point of the surface of the water is equal to the algebraical sum of all the portions of the waves which at that moment there concur.*

It is the same with the waves of sound. They, too, are added together at every point of the mass of air, as well as in contact with the listener's ear. For them also the degree of condensation and the velocity of the particles of air in the passages of the organ of hearing are equal to the algebraical sums of the separate degrees of condensation and of the velocities of the waves of sound, considered apart. This single motion of the air produced by the simultaneous action of various sounding bodies, has now to be analysed by the ear into the separate parts which correspond to their separate effects. For doing this the ear is much more unfavourably situated than the eye. The latter surveys the whole undulating surface at a glance. But the ear can, of course, only perceive the motion of the particles of air which impinge upon it. And yet the ear solves its problem with the greatest exactness, certainty, and determinacy. This power of the ear is of supreme importance for hearing. Were it not present it would be impossible to distinguish different tones.

Some recent anatomical discoveries appear to give a clue to the explanation of this important power of the ear.

You will all have observed the phenomena of the sympathetic production of tones in musical instruments, especially stringed instruments. The string of a pianoforte when the damper is raised begins to vibrate as soon as its proper tone is produced in its neighbourhood with sufficient force by some other means. When this foreign tone ceases the tone of the string will be heard to continue some little time longer. If we put little paper riders on the string they will be jerked off when its tone is thus produced in the neighbourhood. This sympathetic action of the string depends on the impact of the vibrating particles of air against the string and its sounding-board.

Each *separate* wave-crest (or condensation) of air which passes by the string is, of course, too weak to produce a sensible motion in it. But when a long series of wave-crests (or condensations) strike the string in such a manner that each succeeding one increases the slight tremour which resulted from the action of its predecessors, the effect finally becomes sensible. It is a process of exactly the same nature as the swinging of a heavy bell. A powerful man can scarcely move it sensibly by a single impulse. A boy, by pulling the rope at regular intervals corresponding to the time of its oscillations, can gradually bring it into violent motion.

This peculiar reinforcement of vibration depends entirely on the rhythmical application of the impulse. When the bell has been once made to vibrate as a pendulum in a very small arc, and the boy always pulls the rope as it falls, and at a time that his pull augments the existing velocity of the bell, this velocity, increasing slightly at each pull, will gradually become considerable. But if the boy apply his power at irregular intervals, sometimes increasing and sometimes diminishing the motion of the bell, he will produce no sensible effect.

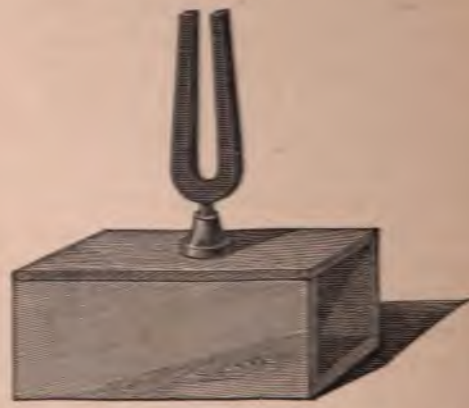
In the same way that a mere boy is thus enabled to swing a heavy bell, the tremours of light and mobile air suffice to set in motion the heavy and solid mass of steel contained in a tuning-fork, provided that the tone which is excited in the air is exactly in unison with that of the fork, because in this case also every impact of a wave of air against the fork increases the motions excited by the like previous blows.

This experiment is most conveniently performed on a fork, Fig. 7, which is fastened to a sounding-board, the air being excited by a similar fork of precisely the same pitch. If one is struck, the other will be found after a few seconds to be sounding also. Then damp the first

fork, by touching it for a moment with a finger, and the second will continue the tone. The second will then bring the first into vibration, and so on.

But if a very small piece of wax be attached to the ends of one of the forks, whereby its pitch will be rendered scarcely perceptibly lower than the other, the sympathetic vibration of the second fork ceases, because the times of oscillation are no longer the same in each. The blows which the waves of air excited by the first inflict upon the sounding board of the second fork, are indeed for a time in the same direction as the motions of

FIG. 7.



the second fork, and consequently increase the latter, but after a very short time they cease to be so, and consequently destroy the slight motion which they had previously excited.

Lighter and more mobile elastic bodies, as for example strings, can be set in motion by a much smaller number of aerial impulses. Hence they can be set in sympathetic motion much more easily than tuning forks, and by means of a musical tone which is far less accurately in unison with themselves.

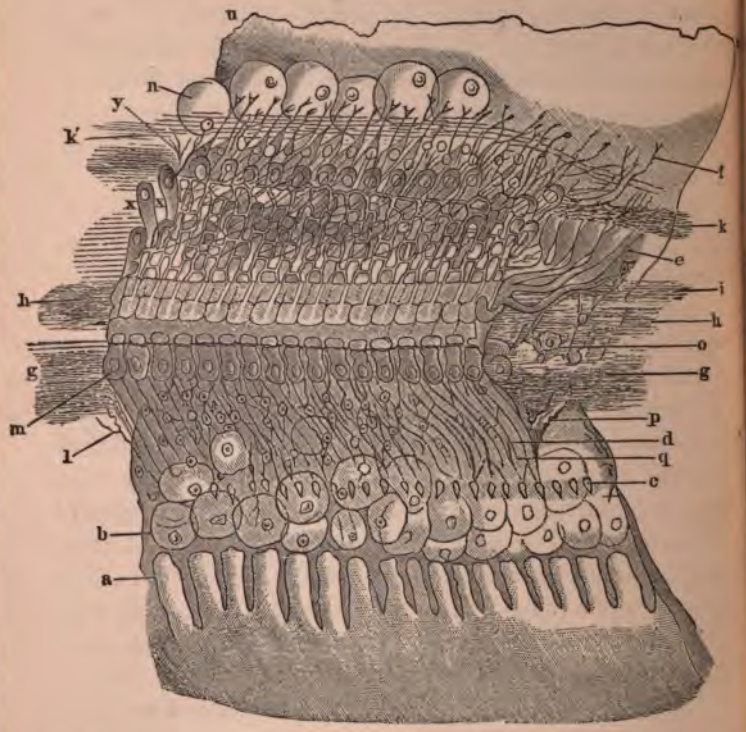
Now, then, if several tones are sounded in the neighbourhood of a pianoforte, no string can be set in sympathetic vibration unless it is in unison with one of those tones. For example, depress the forte pedal (thus raising the dampers), and put paper riders on all the strings. They will of course leap off when their strings are put in vibration. Then let several voices or instruments sound tones in the neighbourhood. All those riders, and *only* those, will leap off which are placed upon strings that correspond to tones of the same pitch as those sounded. You perceive that a pianoforte is also capable of analysing the wave confusion of the air into its elementary constituents.

The process which actually goes on in our ear is probably very like that just described. Deep in the petrous bone out of which the internal ear is hollowed, lies a peculiar organ, the cochlea or snail shell—a cavity filled with water, and so called from its resemblance to the shell of a common garden snail. This spiral passage is divided throughout its length into three sections, upper, middle, and lower, by two membranes stretched in the middle of its height. The Marchese Corti discovered some very remarkable formations in the middle section. They consist of innumerable plates, microscopically small, and arranged orderly side by side, like the keys of a piano. They are connected at one end with the fibres of the auditory nerve, and at the other with the stretched membrane.

Fig. 8 shows this extraordinarily complicated arrangement for a small part of the partition of the cochlea. The arches which leave the membrane at d and are re-inserted at e, reaching their greatest height between m and o, are probably the parts which are suited for vibration. They are spun round with innumerable fibrils, among which some nerve fibres can be recognised, coming to

them through the holes near c. The transverse fibres g, h, i, k, and the cells o, also appear to belong to the nervous system. There are about three thousand arches similar to d e, lying orderly beside each other, like the keys of a piano in the whole length of the partition of the cochlea.

Fig. 8.



In the so-called vestibulum, also, where the nerves expand upon little membranous bags swimming in water, elastic appendages, similar to stiff hairs, have been lately discovered at the ends of the nerves. The anatomical arrangement of these appendages leaves scarcely any

room to doubt that they are set into sympathetic vibration by the waves of sound which are conducted through the ear. Now if we venture to conjecture—it is at present only a conjecture, but after careful consideration I am led to think it very probable—that every such appendage is tuned to a certain tone like the strings of a piano, then the recent experiment with a piano shows you that when (and only when) that tone is sounded the corresponding hair-like appendage may vibrate, and the corresponding nerve-fibre experience a sensation, so that the presence of each single such tone in the midst of a whole confusion of tones must be indicated by the corresponding sensation.

Experience then shows us that the ear really possesses the power of analysing waves of air into their elementary forms.

By compound motions of the air, we have hitherto meant such as have been caused by the simultaneous vibration of several elastic bodies. Now, since the forms of the waves of sound of different musical instruments are different, there is room to suppose that the kind of vibration excited in the passages of the ear by one such tone will be exactly the same as the kind of vibration which in another case is there excited by two or more instruments sounded together. If the ear analyses the motion into its elements in the latter case, it cannot well avoid doing so in the former, where the tone is due to a single source. And this is found to be really the case.

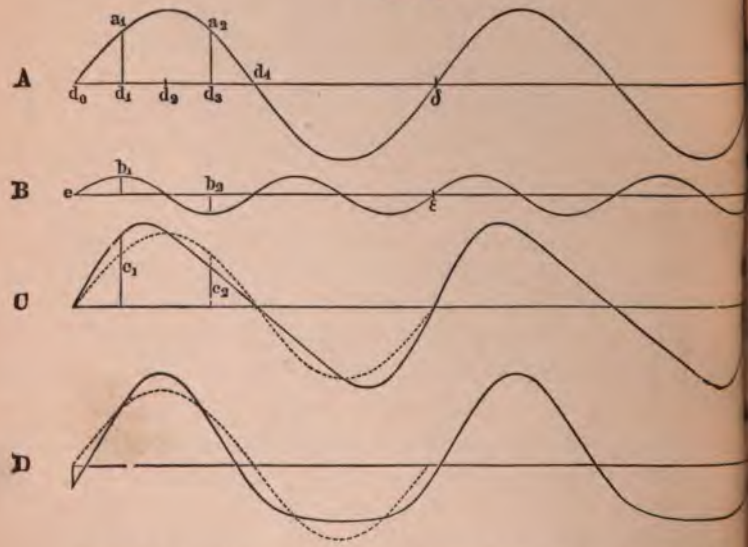
I have previously mentioned the form of wave with gently rounded crests and hollows, and termed it simple or pure (p. 75). In reference to this form the French mathematician Fourier has established a celebrated and important theorem which may be translated from mathematical into ordinary language thus: *Any form of wave whatever can be compounded of a number of simple waves of*

different lengths. The longest of these simple waves has the same length as that of the given form of wave, the others have lengths one-half, one-third, one-fourth, &c. as great.

By the different modes of uniting the crests and hollows of these simple waves, an endless multiplicity of wave-forms may be produced.

For example, the wave-curves A and B, Fig. 9, represent

FIG. 9.

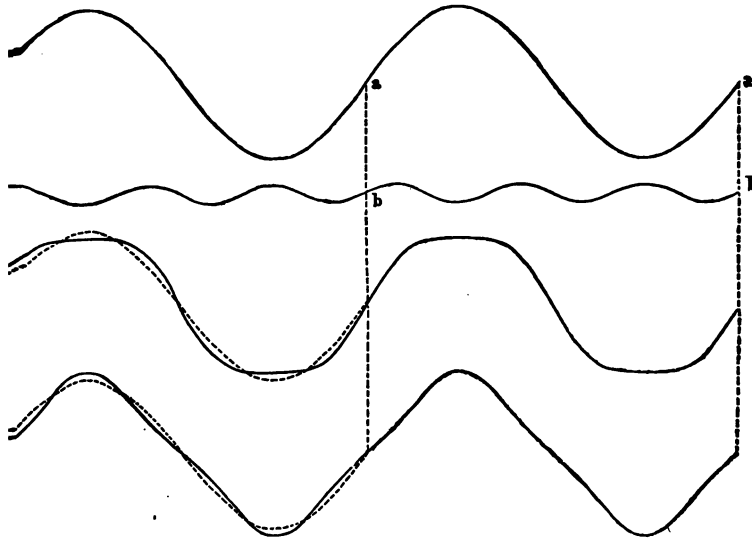


waves of simple tones, B making twice as many vibrations as A in a second of time, and being consequently an octave higher in pitch. C and D, on the other hand, represent the waves which result from the superposition of B on A. The dotted curves in the first halves of C and D are repetitions of so much of the figure A. In C, the initial point e of the curve B coincides with the initial point d_0 of A. But in D, the deepest point b_2 of the first hollow in B is placed under the initial point of A. The result is two different compound-curves, the first C having steeply ascending

and more gently descending crests, but so related that, by reversing the figure, the elevations would exactly fit into the depressions. But in D we have pointed crests and flattened hollows, which are, however, symmetrical with respect to right and left.

Other forms are shown in Fig. 10, which are also compounded of two simple waves, A and B, of which B makes three times as many vibrations in a second as A, and consequently is the

Fig. 10.



twelfth higher in pitch. The dotted curves in C and D are, as before, repetitions of A. C has flat crests and flat hollows, D has pointed crests and pointed hollows.

These extremely simple examples will suffice to give a conception of the great multiplicity of forms resulting from this method of composition. Supposing that instead of two, several simple waves were selected, with heights and initial points arbitrarily chosen, an endless variety of changes could be

effected, and, in point of fact, any given form of wave could be reproduced.[1]

When various simple waves concur on the surface of water, the compound wave-form has only a momentary existence, because the longer waves move faster than the shorter, and consequently the two kinds of wave immediately separate, giving the eye an opportunity of recognising the presence of several systems of waves. But when waves of sound are similarly compounded, they never separate again, because long and short waves traverse air with the same velocity. Hence the compound wave is permanent, and continues its course unchanged, so that when it strikes the ear, there is nothing to indicate whether it originally left a musical instrument in this form, or whether it had been compounded on the way, out of two or more undulations.

Now what does the ear do? Does it analyse this compound wave? Or does it grasp it as a whole? The answer to these questions depends upon the sense in which we take them. We must distinguish two different points—the audible *sensation*, as it is developed without any intellectual interference, and the *conception*, which we form in consequence of that sensation. We have, as it were, to distinguish between the material ear of the body and the spiritual ear of the mind. The material ear does precisely what the mathematician effects by means of Fourier's theorem, and what the pianoforte accomplishes when a confused mass of tones is presented to it. It analyses those wave-forms which were not originally due to simple undulations, such as those furnished by tuning forks, into a sum of simple tones, and

[1] Of course the waves could not overhang, but waves of such a form would have no possible analogue in waves of sound [which the reader will recollect are not actually in the forms here drawn, but have only condensations and rarefactions, conveniently replaced by these forms, p. 73].

feels the tone due to each separate simple wave separately, whether the compound wave originally proceeded from a source capable of generating it, or became compounded on the way.

For example, on striking a string, it will give a tone corresponding, as we have seen, to a wave-form widely different from that of a simple tone. When the ear analyses this wave-form into a sum of simple waves, it hears at the same time a series of simple tones corresponding to these waves.

Strings are peculiarly favourable for such an investigation, because they are themselves capable of assuming extremely different forms in the course of their vibration, and these forms may also be considered, like those of aerial undulations, as compounded of simple waves. Fig. 4, p. 76, shows the consecutive forms of a string struck by a simple rod. Fig. 11, p. 90, gives a number of other forms of vibration of a string, corresponding to simple tones. The continuous line shows the extreme displacement of the string in one direction, and the dotted line in the other. At a the string produces its fundamental tone, the deepest simple tone it can produce, vibrating in its whole length, first on one side and then on the other. At b it falls into two vibrating sections, separated by a single stationary point β, called a *node* (knot). The tone is an octave higher, the same as each of the two sections would separately produce, and it performs twice as many vibrations in a second as the fundamental tone. At c we have two nodes, γ_1 and γ_2, and three vibrating sections, each vibrating three times as fast as the fundamental tone and hence giving its twelfth. At d_1 there are three nodes, δ_1, δ_2, δ_3, and four vibrating sections, each vibrating four times as quickly as the fundamental tone, and giving the second octave above it.

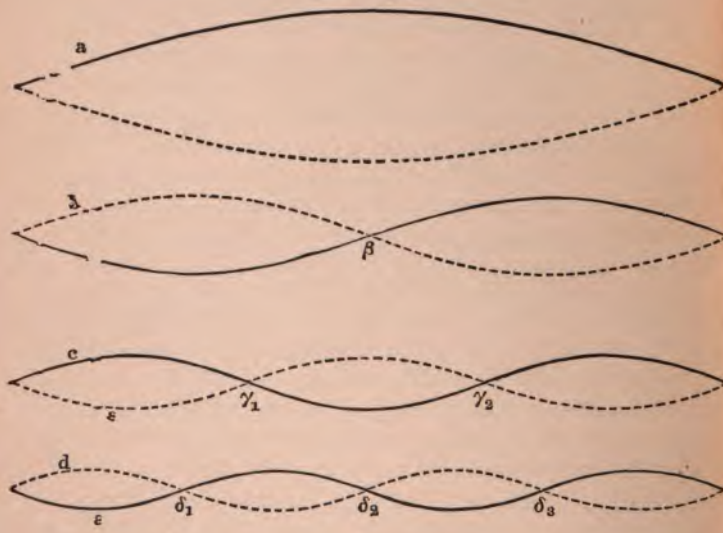
In the same way forms of vibration may occur with 5, 6, 7, &c., vibrating sections, each performing respectively, 5, 6, 7, &c. times as many vibrations in a second as the fundamental tone, and all other vibrational forms of the string may be conceived as compounded of a sum of such simple vibrational forms.

The vibrational forms with stationary points or nodes may be produced, by gently touching the string at one of these points, either with the finger or a rod, and rubbing the string with a

violin bow, plucking it with the finger, or striking it with a pianoforte hammer. The bell-like harmonics or flageolet-tones of strings, so much used in violin playing, are thus produced.

Now suppose that a string has been excited, and after its tone has been allowed to continue for a moment, it is touched gently at its middle point β, Fig. 11 b, or δ_2, Fig. 11 d. The vibrational forms a and c, for which this point is in motion, will be immediately checked and destroyed; but the vibrational forms b and d, for which this point is at rest, will not be disturbed, and the tones due to them will continue to be heard. In

FIG. 11.

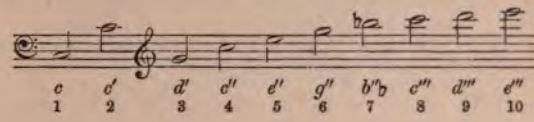


this way we can readily discover whether certain members of the series of simple tones are contained in the compound tone of a string when excited in any given way, and the ear can be rendered sensible of their existence.

When once these simple tones in the sound of a string have been thus rendered audible, the ear will readily be able to observe them in the untouched string, after a little accurate attention.

The series of tones which are thus made to combine with a

given fundamental tone, is perfectly determinate. They are tones which perform twice, thrice, four times, &c., as many vibrations in a second as the fundamental tone. They are called the *upper partials*, or harmonic overtones, of the fundamental tone. If this last be *c*, the series may be written as follows in musical notation, [it being understood that, on account of the temperament of a piano, these are not precisely the fundamental tones of the corresponding strings on that instrument, and that in particular the upper partial, $b''\flat$, is necessarily much flatter than the fundamental tone of the corresponding note on the piano].



Not only strings, but almost all kinds of musical instruments, produce waves of sound which are more or less different from those of simple tones, and are therefore capable of being compounded out of a greater or less number of simple waves. The ear analyses them all by means of Fourier's theorem better than the best mathematician, and on paying sufficient attention can distinguish the separate simple tones due to the corresponding simple waves. This corresponds precisely to our theory of the sympathetic vibration of the organs described by Corti. Experiments with the piano, as well as the mathematical theory of sympathetic vibrations, show that any upper partials which may be present will also produce sympathetic vibrations. It follows, therefore, that in the cochlea of the ear, every external tone will set in sympathetic vibration, not merely the little plates with their accompanying nerve-fibres, corresponding to its fundamental tone, but also those corresponding to all the upper partials, and that consequently the latter must be heard as well as the former.

Hence a simple tone is one excited by a succession of

simple wave-forms. All other wave-forms, such as those produced by the greater number of musical instruments, excite sensations of a variety of simple tones.

Consequently, all the tones of musical instruments must in strict language, so far as the sensation of musical tone is concerned, be regarded as chords with a predominant fundamental tone.

The whole of this theory of upper partials or harmonic overtones will perhaps seem new and singular. Probably few or none of those present, however frequently they may have heard or performed music, and however fine may be their musical ear, have hitherto perceived the existence of any such tones, although, according to my representations, they must be always and continuously present. In fact, a peculiar act of attention is requisite in order to hear them, and unless we know how to perform this act, the tones remain concealed. As you are aware, no perceptions obtained by the senses are merely sensations impressed on our nervous systems. A peculiar intellectual activity is required to pass from a nervous sensation to the conception of an external object, which the sensation has aroused. The sensations of our nerves of sense are mere symbols indicating certain external objects, and it is usually only after considerable practice that we acquire the power of drawing correct conclusions from our sensations respecting the corresponding objects. Now it is a universal law of the perceptions obtained through the senses, that we pay only so much attention to the sensations actually experienced, as is sufficient for us to recognise external objects. In this respect we are very onesided and inconsiderate partisans of practical utility; far more so indeed than we suspect. All sensations which have no direct reference to external objects, we are accustomed, as a matter of course, entirely to ignore, and we do not become aware of them till we make a scientific

investigation of the action of the senses, or have our attention directed by illness to the phenomena of our own bodies. Thus we often find patients, when suffering under a slight inflammation of the eyes, become for the first time aware of those beads and fibres known as *mouches volantes* swimming about within the vitreous humour of the eye, and then they often hypochondriacally imagine all sorts of coming evils, because they fancy that these appearances are new, whereas they have generally existed all their lives.

Who can easily discover that there is an absolutely blind point, the so-called *punctum cæcum*, within the retina of every healthy eye? How many people know that the only objects they see single are those at which they are looking, and that all other objects, behind or before these, appear double? I could adduce a long list of similar examples, which have not been brought to light till the actions of the senses were scientifically investigated, and which remain obstinately concealed, till attention has been drawn to them by appropriate means —often an extremely difficult task to accomplish.

To this class of phenomena belong the upper partial tones. It is not enough for the auditory nerve to have a sensation. The intellect must reflect upon it. Hence my former distinction of a material and a spiritual ear.

We always hear the tone of a string accompanied by a certain combination of upper partial tones. A different combination of such tones belongs to the tone of a flute, or of the human voice, or of a dog's howl. Whether a violin or a flute, a man or a dog is close by us is a matter of interest for us to know, and our ear takes care to distinguish the peculiarities of their tones with accuracy. The *means* by which we can distinguish them, however, is a matter of perfect indifference.

Whether the cry of the dog contains the higher octave

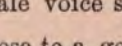
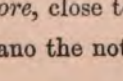
or the twelfth of the fundamental tone, has no practical interest for us, and never occupies our attention. The upper partials are consequently thrown into that unanalysed mass of peculiarities of a tone which we call its *quality*. Now as the existence of upper partial tones depends on the *wave form*, we see, as I was able to state previously (p. 74), that the *quality of tone* corresponds to the *form of wave*.

The upper partial tones are most easily heard when they are not in harmony with the fundamental tone, as in the case of bells. The art of the bell-founder consists precisely in giving bells such a form that the deeper and stronger partial tones shall be in harmony with the fundamental tone, as otherwise the bell would be unmusical, tinkling like a kettle. But the higher partials are always out of harmony, and hence bells are unfitted for artistic music.

On the other hand, it follows, from what has been said, that the upper partial tones are all the more difficult to hear, the more accustomed we are to the compound tones of which they form a part. This is especially the case with the human voice, and many skilful observers have consequently failed to discover them there.

The preceding theory was wonderfully corroborated by leading to a method by which not only I myself, but other persons, were enabled to hear the upper partial tones of the human voice.

No particularly fine musical ear is required for this purpose, as was formerly supposed, but only proper means for directing the attention of the observer.

Let a powerful male voice sing the note $e\flat$  to the vowel *o* in *ore*, close to a good piano. Then lightly touch on the piano the note $b'\flat$  in the next octave

above, and listen attentively to the sound of the piano as it dies away. If this $b'\flat$ is a real upper partial in the compound tone uttered by the singer, the sound of the piano will apparently not die away at all, but the corresponding upper partial of the voice will be heard as if the note of the piano continued.[1] By properly varying the experiment, it will be found possible to distinguish the vowels from one another by their upper partial tones.

The investigation is rendered much easier by arming the ear with small globes of glass or metal, as in Fig. 12.

Fig. 12.

The larger opening a is directed to the source of sound, and the smaller funnel-shaped end is applied to the drum of the ear. The enclosed mass of air, which is almost entirely separated from that without, has its own proper tone or key-note, which will be heard, for example, on blowing across the edge of the opening a. If then this proper tone of the globe is excited in the external air, either as a fundamental or upper partial tone, the in-

[1] In repeating this experiment the observer must remember that the $e\flat$ of the piano is not a true twelfth below the $b'\flat$. Hence the singer should first be given $b'\flat$ from the piano, which he will naturally sing as $b\flat$, an octave lower, and then take a true fifth below it. A skilful singer will thus hit the true twelfth and produce the required upper partial $b'\flat$. On the other hand, if he sings $e\flat$ from the piano, his upper partial $b'\flat$ will probably beat with that of the piano.—Tr.

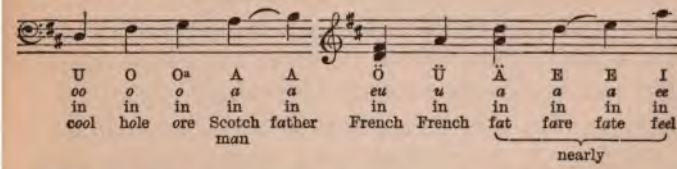
cluded mass of air is brought into violent sympathetic vibration, and the ear thus connected with it hears the corresponding tone with much increased intensity. By this means it is extremely easy to determine whether the proper tone of the globe is or is not contained in a compound tone or mass of tones.

On examining the vowels of the human voice, it is easy to recognise, with the help of such resonators as have just been described, that the upper partial tones of each vowel are peculiarly strong in certain parts of the scale: thus O in *ore* has its upper partials in the neighbourhood of $b'\flat$, A in *father* in the neighbourhood of $b''\flat$ (an octave higher). The following gives a general view of those portions of the scale where the upper partials of the vowels, as pronounced in the north of Germany, are particularly strong.

[1] The corresponding English vowel sounds are probably none of them precisely the same as those pronounced by the author. It is necessary to note this, for a very slight variation in pronunciation would produce a change in the fundamental tone, and consequently a more considerable change in the position of the upper partials. The tones given by Donders, which are written below the English equivalents, are cited on the authority of Helmholtz's *Tonempfindungen*, 3rd edition, 1870, p. 171, where Helmholtz says: 'Donders's results differ somewhat from mine, partly because his refer to a Dutch, and mine to a North German pronunciation, and partly because Donders, not having had the assistance of tuning forks, could not always correctly determine the octave to which the sounds belong.' Also (*ib.* p. 167) the author remarks that $b''\flat$ answers only to the deep German *a* (which is the broad Scotch *a'*, or *aw* without labialisation), and that if the brighter Italian *a* (English *a* in *father*) be used, the resonance rises a third, to d'''. Dr. C. L. Merkel, of Leipzig, in his *Physiologie der menschlichen*

The following easy experiment clearly shows that it is indifferent whether the several simple tones contained in a compound tone like a vowel uttered by the human voice come from one source or several. If the dampers of a pianoforte are raised, not only do the sympathetic vibrations of the strings furnish tones of the same *pitch* as those uttered beside it; but if we sing A (*a* in *father*) to any note of the piano, we hear an A quite clearly returned from the strings; and if E (*a* in *fare* or *fate*), O (*o* in *hole* or *ore*), and U (*oo* in *cool*), be similarly sung to the note, E, O, and U will also be echoed back. It is only necessary to hit the note of the piano with great exactness.[1] Now the sound of the vowel is produced

Sprache, 1866, p. 109, after citing Helmholtz's experiments as detailed in his *Tonempfindungen*, gives the following as 'the pitches of the vowels according to his most recent examination of his own habits of speech, as accurately as he is able to note them.'



'Here the note *a* applies to the *timbre obscur* of A with low larynx, and *b* to the *timbre clair* of A with high larynx, and similarly the vowel E may pass from *d''* to *e'* by narrowing the channel in the mouth. The intermediate vowels Ö, Ä, have also two different timbres and hence their pitch is not fixed; the most frequent are consequently written over one another; the lower note is for the obscure, and the higher for the bright timbre. But the vowel Ü seems to be tolerably fixed as *a'*, just as its parents U and I are upon *d* and *a''*, and it has consequently the pitch of the ordinary *a'* tuning fork.'—Tr.

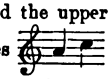
[1] My own experience shows that if any vowel at any pitch be loudly and sharply sung, or called out, beside a piano, of which the dampers have been raised, that vowel will be echoed back. There is generally a sensible pause before the echo is heard. Before repeating the experiment with a new vowel, whether at the same or a different pitch, damp all the strings and then again raise the dampers. The result can easily be made audible to a hundred persons at once, and it is extremely interesting and instructive. It is peculiarly so, if different vowels be sung to the same pitch, so that they

solely by the sympathetic vibration of the higher strings, which correspond with the upper partial tones of the tone sung.

In this experiment the tones of numerous strings are excited by a tone proceeding from a single source, the human voice, which produces a motion of the air, equivalent in form, and therefore in quality, to that of this single tone itself.

We have hitherto spoken only of compositions of waves of different lengths. We will now compound waves of the same length which are moving in the same direction. The result will be entirely different, according as the elevations of one coincide with those of the other (in which case elevations of double the height and depressions of double the depth are produced), or the elevations of one fall on the *depressions* of the other. If both waves have the same height, so that the elevations of one exactly fit into the depressions of the other, both elevations and depressions will vanish in the second case, and the two waves will mutually destroy each other. Similarly two waves of sound, as well as two waves of water, may mutually destroy each other, when the condensations of one coincide with the rarefactions of the other. This remarkable phenomenon wherein sound is silenced by a precisely similar sound, is called the *interference* of sounds.

This is easily proved by means of the siren already described. On placing the upper box so that the puffs of air may proceed simultaneously from the rows of twelve holes in each wind chest, their effect is reinforced, and

have all the same fundamental tone, and the upper partials only differ in intensity. For female voices the pitches  a' to c'' are favourable for all vowels. This is a fundamental experiment for the theory of vowel sounds, and should be repeated by all who are interested in speech.—Tr.

we obtain the fundamental tone of the corresponding tone of the siren very full and strong. But on arranging the boxes so that the upper puffs escape when the lower series of holes is covered, and conversely, the fundamental tone vanishes, and we only hear a faint sound of the first upper partial, which is an octave higher, and which is not destroyed by interference under these circumstances.

Interference leads us to the so-called musical beats. If two tones of *exactly* the same pitch are produced simultaneously, and their elevations coincide at first, they will never cease to coincide, and if they did not coincide at first they never will coincide.

The two tones will either perpetually reinforce, or perpetually destroy each other. But if the two tones have only *approximatively* equal pitches, and their elevations at first coincide, so that they mutually reinforce each other, the elevations of one will gradually outstrip the elevations of the other. Times will come when the elevations of the one fall upon the depressions of the other, and then other times when the more rapidly advancing elevations of the one will have again reached the elevations of the other. These alternations become sensible by that alternate increase and decrease of loudness, which we call a *beat*. These beats may often be heard when two instruments which are not exactly in unison play a note of the same name. When the two or three strings which are struck by the same hammer on a piano are out of tune, the beats may be distinctly heard. Very slow and regular beats often produce a fine effect in sostenuto passages, as in sacred part-songs, by pealing through the lofty aisles like majestic waves, or by a gentle tremour giving the tone a character of enthusiasm and emotion. The greater the difference of the pitches, the quicker the beats. As long as no more than four to six beats occur in a second, the ear readily distinguishes the alternate reinforcements

of the tone. If the beats are more rapid the tone grates on the ear, or, if it is high, becomes cutting. A grating tone is one interrupted by rapid breaks, like that of the letter R, which is produced by interrupting the tone of the voice by a tremour of the tongue or uvula.[1]

When the beats become more rapid, the ear finds a continually-increasing difficulty when attempting to hear them separately, even though there is a sensible roughness of the tone. At last they become entirely undistinguishable, and, like the separate puffs which compose a tone, dissolve as it were into a continuous sensation of tone.[2]

Hence, while every separate musical tone excites in the auditory nerve a uniform sustained sensation, two tones of different pitches mutually disturb one another, and split up into separable beats, which excite a feeling of discontinuity as disagreeable to the ear as similar intermittent but rapidly repeated sources of excitement are unpleasant to the other organs of sense; for example, flickering and glittering light to the eye, scratching with a brush to the skin. This roughness of tone is the essential character of dissonance. It is most unpleasant to the ear when the two tones differ by about a semitone, in which case, in the middle portions of the scale, from twenty to forty beats ensue in a second. When the difference is a whole tone, the roughness is less; and when it reaches a *third* it usually disappears, at least in the higher parts of the scale. The (minor or major) *third* may in conse-

[1] The trill of the uvula is called the Northumbrian burr, and is not known out of Northumberland, in England. In France it is called the *r grasseyé* or *provençal*, and is the commonest Parisian sound of *r*. The uvula trill is also very common in Germany, but it is quite unknown in Italy.—Tr.

[2] The transition of beats into a harsh dissonance was displayed by means of two organ pipes, of which one was gradually put more and more out of tune with the other.

quence pass as a consonance. Even when the fundamental tones have such widely-different pitches that they cannot produce audible beats, the upper partial tones may beat and make the tone rough. Thus, if two tones form a *fifth* (that is, one makes two vibrations in the same time as the other makes three), there is one upper partial in both tones which makes six vibrations in the same time. Now, if the ratio of the pitches of the fundamental tones is exactly as 2 to 3, the two upper partial tones of six vibrations are precisely alike, and do not destroy the harmony of the fundamental tones. But if this ratio is only approximatively as 2 to 3, then these two upper partials are not exactly alike, and hence will beat and roughen the tone.

It is very easy to hear the beats of such imperfect fifths, because, as our pianos and organs are now tuned, all the fifths are impure, although the beats are very slow. By properly directed attention, or still better with the help of a properly tuned resonator, it is easy to hear that it is the particular upper partials here spoken of, that are beating together. The beats are necessarily weaker than those of the fundamental tones, because the beating upper partials are themselves weaker. Although we are not usually clearly conscious of these beating upper partials, the ear feels their effect as a want of uniformity or a roughness in the mass of tone, whereas a perfectly pure fifth, the pitches being precisely in the ratio of 2 to 3, continues to sound with perfect smoothness, without any alterations, reinforcements, diminutions, or roughnesses of tone. As has already been mentioned, the siren proves in the simplest manner that the most perfect consonance of the fifth precisely corresponds to this ratio between the pitches. We have now learned the reason of the roughness experienced when any deviation from that ratio has been produced.

In the same way two tones, which have their pitches exactly in the ratios of 3 to 4, or 4 to 5, and consequently form a perfect fourth or a perfect major third, sound much better when sounded together, than two others of which the pitches slightly deviate from this exact ratio. In this manner, then, any given tone being assumed as fundamental, there is a precisely determinate number of other degrees of tone which can be sounded at the same time with it, without producing any want of uniformity or any roughness of tone, or which will at least produce less roughness than any slightly greater or smaller intervals of tone under the same circumstances.

This is the reason why modern music, which is essentially based on the harmonious consonance of tones, has been compelled to limit its scale to certain determinate degrees. But even in ancient music, which allowed only one part to be sung at a time, and hence had no harmony in the modern sense of the word, it can be shown that the upper partial tones contained in all musical tones sufficed to determine a preference in favour of progressions through certain determinate intervals. When an upper partial tone is common to two successive tones in a melody, the ear recognises a certain relationship between them, serving as an artistic bond of union. Time is, however, too short for me to enlarge on this topic, as we should be obliged to go far back into the history of music.

I will but mention that there exists another kind of secondary tones, which are only heard when two or more loudish tones of different pitch are sounded together, and are hence termed *combinational*.[1] These secondary tones

[1] These are of two kinds, *differential* and *summational*, according as their pitch is the difference or sum of the pitches of the two generating tones. The former are the only combinational tones here spoken of. The discovery of the latter was entirely due to the theoretical investigations of the author.—Tr.

are likewise capable of beating, and hence producing roughness in the chords. Suppose a perfectly just major third c' e' (ratio of pitches, 4 to 5) is sounded on the siren, or with properly-tuned organ pipes, or on a violin;[1] then a faint C two octaves deeper than the c' will be heard as a combinational tone. The same C is also heard when the tones e' g' (ratio of pitches 5 to 6) are sounded together.[2]

If the three tones c', e', g', having their pitches precisely in the ratios 4, 5, and 6, are struck together, the combinational tone C is produced twice[3] in perfect unison, and without beats. But if the three notes are not exactly thus tuned,[4] the two C combinational tones will have different pitches, and produce faint beats.

The combinational tones are usually much weaker than the upper partial tones, and hence their beats are much less rough and sensible than those of the latter. They are consequently but little observable, except in tones which have scarcely any upper partials, as those produced by flutes or the closed pipes of organs. But it is indisputable that on such instruments part-music scarcely presents any line of demarcation between harmony and dysharmony, and is consequently deficient both in strength and character. On the contrary, all good musical qualities of tone are comparatively rich in upper partials, possessing

[1] In the ordinary tuning of the English concertina this major third is just, and generally this instrument shows the differential tones very well. The major third is very false on the harmonium and piano.—Tr.

[2] This minor third is very false on the English concertina, harmonium, or piano, and the combinational tone heard is consequently very different from the true C.—Tr.

[3] The combinational tone c, an octave higher, is also produced once from the fifth c' g'.—Tr.

[4] As on the English concertina or harmonium, on both of which the consequent effect may be well heard.—Tr.

the five first, which form the octaves, fifths, and major thirds of the fundamental tone. Hence, in the mixture stops of the organ, additional pipes are used, giving the series of upper partial tones corresponding to the pipe producing the fundamental tone, in order to generate a penetrating, powerful quality of tone to accompany congregational singing. The important part played by the upper partial tones in all artistic musical effects is here also indisputable.

We have now reached the heart of the theory of harmony. Harmony and dysharmony are distinguished by the undisturbed current of the tones in the former, which are as flowing as when produced separately, and by the disturbances created in the latter, in which the tones split up into separate beats. All that we have considered, tends to this end. In the first place the phenomenon of beats depends on the interference of waves. Hence they could only occur, if sound were due to undulations. Next, the determination of consonant intervals necessitated a capability in the ear of feeling the upper partial tones, and analysing the compound systems of waves into simple undulations, according to Fourier's theorem. It is entirely due to this theorem that the pitches of the upper partial tones of all serviceable musical tones must stand to the pitch of their fundamental tones in the ratios of the whole numbers to 1, and that consequently the ratios of the pitches of concordant intervals must correspond with the smallest possible whole numbers. How essential is the physiological constitution of the ear which we have just considered, becomes clear by comparing it with that of the eye. Light is also an undulation of a peculiar medium, the luminous ether, diffused through the universe, and light, as well as sound, exhibits phenomena of interference. Light, too, has waves of various periodic

times of vibration, which produce in the eye the sensation of colour, red having the greatest periodic time, then orange, yellow, green, blue, violet; the periodic time of violet being about half that of the outermost red. But the eye is unable to decompose compound systems of luminous waves, that is, to distinguish compound colours from one another. It experiences from them a single, unanalysable, simple sensation, that of a mixed colour. It is indifferent to the eye whether this mixed colour results from a union of fundamental colours with simple, or with non-simple ratios of periodic times. The eye has no sense of harmony in the same meaning as the ear. There is no music to the eye.

Æsthetics endeavour to find the principle of artistic beauty in its unconscious conformity to law. To-day I have endeavoured to lay bare the hidden law, on which depends the agreeableness of consonant combinations. It is in the truest sense of the word unconsciously obeyed, so far as it depends on the upper partial tones, which, though felt by the nerves, are not usually consciously present to the mind. Their compatibility or incompatibility however is felt, without the hearer knowing the cause of the feeling he experiences.

These phenomena of agreeableness of tone, as determined solely by the senses, are of course merely the first step towards the beautiful in music. For the attainment of that higher beauty which appeals to the intellect, harmony and dysharmony are only means, although essential and powerful means. In dysharmony the auditory nerve feels hurt by the beats of incompatible tones. It longs for the pure efflux of the tones into harmony. It hastens towards that harmony for satisfaction and rest. Thus both harmony and dysharmony alternately urge and moderate the flow of tones, while the mind sees in their immaterial motion an image of its own perpetually

streaming thoughts and moods. Just as in the rolling ocean, this movement, rhythmically repeated, and yet ever varying, rivets our attention and hurries us along. But whereas in the sea, blind physical forces alone are at work, and hence the final impression on the spectator's mind is nothing but solitude—in a musical work of art the movement follows the outflow of the artist's own emotions. Now gently gliding, now gracefully leaping, now violently stirred, penetrated or laboriously contending with the natural expression of passion, the stream of sound, in primitive vivacity, bears over into the hearer's soul unimagined moods which the artist has overheard from his own, and finally raises him up to that repose of everlasting beauty, of which God has allowed but few of his elect favourites to be the heralds.

But I have reached the confines of physical science, and must close.

ICE AND GLACIERS.

A LECTURE DELIVERED AT FRANKFORT-ON-THE-MAIN, AND AT HEIDELBERG, IN FEBRUARY 1865.

THE world of ice and of eternal snow, as unfolded to us on the summits of the neighbouring Alpine chain, so stern, so solitary, so dangerous, it may be, has yet its own peculiar charm. Not only does it enchain the attention of the natural philosopher, who finds in it the most wonderful disclosures as to the present and past history of the globe, but every summer it entices thousands of travellers of all conditions, who find there mental and bodily recreation. While some content themselves with admiring from afar the dazzling adornment which the pure, luminous masses of snowy peaks, interposed between the deeper blue of the sky and the succulent green of the meadows, lend to the landscape, others more boldly penetrate into the strange world, willingly subjecting themselves to the most extreme degrees of exertion and danger, if only they may fill themselves with the aspect of its sublimity.

I will not attempt what has so often been attempted in vain—to depict in words the beauty and magnificence of nature, whose aspect delights the Alpine traveller. I may well presume that it is known to most of you from your own observation; or, it is to be hoped, will be so. But I imagine that the delight and interest in

magnificence of those scenes will make you the more inclined to lend a willing ear to the remarkable results of modern investigations on the more prominent phenomena of the glacial world. There we see that minute peculiarities of ice, the mere mention of which might at other times be regarded as a scientific subtlety, are the causes of the most important changes in glaciers; shapeless masses of rock begin to relate their histories to the attentive observer, histories which often stretch far beyond the past of the human race into the obscurity of the primeval world; a peaceful, uniform, and beneficent sway of enormous natural forces, where at first sight only desert wastes are seen, either extended indefinitely in cheerless, desolate solitudes, or full of wild, threatening confusion—an arena of destructive forces. And thus I think I may promise that the study of the connection of those phenomena of which I can now only give you a very short outline will not only afford you some prosaic instruction, but will make your pleasure in the magnificent scenes of the high mountains more vivid, your interest deeper, and your admiration more exalted.

Let me first of all recall to your remembrance the chief features of the external appearance of the snow-fields and of the glaciers; and let me mention the accurate measurements which have contributed to supplement observation, before I pass to discuss the causal connection of those processes.

The higher we ascend the mountains the colder it becomes. Our atmosphere is like a warm covering spread over the earth; it is well-nigh entirely transparent for the luminous darting rays of the sun, and allows them to pass almost without appreciable change. But it is not equally penetrable by obscure heat-rays, which, proceeding from heated terrestrial bodies, struggle to diffuse themselves into space. These are absorbed by atmospheric air, especially when it

is moist; the mass of air is itself heated thereby, and only radiates slowly into space the heat which has been gained. The expenditure of heat is thus retarded as compared with the supply, and a certain store of heat is retained along the whole surface of the earth. But on high mountains the protective coating of the atmosphere is far thinner—the radiated heat of the ground can escape thence more freely into space; there, accordingly, the accumulated store of heat and the temperature are far smaller than at lower levels.

To this must be added another property of air which acts in the same direction. In a mass of air which expands, part of its store of heat disappears; it becomes cooler, if it cannot acquire fresh heat from without. Conversely, by renewed compression of the air, the same quantity of heat is reproduced which had disappeared during expansion. Thus if, for instance, south winds drive the warm air of the Mediterranean towards the north, and compel it to ascend along the great mountain-wall of the Alps, where the air, in consequence of the diminished pressure, expands by about half its volume, it thereby becomes very greatly cooled—for a mean height of 11,000 feet, by from 18° to 30° C., according as it is moist or dry—and it thereby deposits the greater part of its moisture as rain or snow. If the same wind, passing over to the north side of the mountains as Föhn-wind, reaches the valleys and plains, it again becomes condensed, and is again heated. Thus the same current of air which is warm in the plains, both on this side of the chain and on the other, is bitterly cold on the heights, and can there deposit snow, while in the plain we find it insupportably hot.

The lower temperature at greater heights, which is due to both these causes, is, as we know, very marked on the lower mountain chains of our neighbourhood. In central Europe it amounts to about 1° C. for an ascent of 480 feet;

in winter it is less—1° for about 720 feet of ascent. In the Alps the differences of temperature at great heights are accordingly far more considerable, so that upon the higher parts of their peaks and slopes the snow which has fallen in winter no longer melts in summer. This line, above which snow covers the ground throughout the entire year, is well known as the *snow-line;* on the northern side of the Alps it is about 8,000 feet high, on the southern side about 8,800 feet. Above the snow-line it may on sunny days be very warm; the unrestrained radiation of the sun, increased by the light reflected from the snow, often becomes utterly unbearable; so that the tourist of sedentary habits, apart from the dazzling of his eyes, which he must protect by dark spectacles or by a veil, usually gets severely sunburnt in the face and hands, the result of which is an inflammatory swelling of the skin and great blisters on the surface. More pleasant testimonies to the power of the sunshine are the vivid colours and the powerful odour of the small Alpine flowers which bloom in the sheltered rocky clefts among the snow-fields. Notwithstanding the powerful radiation of the sun the temperature of the air above the snow-fields only rises to 5°, or at most 8°; this, however, is sufficient to melt a tolerable amount of the superficial layers of snow. But the warm hours and days are too short to overpower the great masses of snow which have fallen during colder times. Hence the height of the snow-line does not depend merely on the temperature of the mountain slope, but also essentially on the amount of the yearly snow-fall. It is lower, for instance, on the moist and warm south slope of the Himalayas, than on the far colder but also far drier north slope of the same mountain. Corresponding to the moist climate of western Europe, the snow-fall upon the Alps is very great, and hence the number and extent of their glaciers are comparatively considerable, so

that few mountains of the earth can be compared with them in this respect. Such a development of the glacial world is, as far as we know, met with only on the Himalayas, favoured by the greater height; in Greenland and in Northern Norway, owing to the colder climate; in a few islands in Iceland; and in New Zealand, from the more abundant moisture.

Places above the snow-line are thus characterised by the fact that the snow which in the course of the year falls on its surface, does not quite melt away in summer, but remains to some extent. This snow, which one summer has left, is protected from the further action of the sun's heat by the fresh quantities that fall upon it during the next autumn, winter, and spring. Of this new snow also next summer leaves some remains, and thus year by year fresh layers of snow are accumulated one above the other. In those places where such an accumulation of snow ends in a steep precipice, and its inner structure is thereby exposed, the regularly stratified yearly layers are easily recognised.

But it is clear that this accumulation of layer upon layer cannot go on indefinitely, for otherwise the height of the snow peak would continually increase year by year. But the more the snow is accumulated the steeper are the slopes, and the greater the weight which presses upon the lower and older layers and tries to displace them. Ultimately a state must be reached in which the snow slopes are too steep to allow fresh snow to rest upon them, and in which the burden which presses the lower layers downwards is so great that these can no longer retain their position on the sides of the mountain. Thus, part of the snow which had originally fallen on the higher regions of the mountain above the snow-line, and had there been protected from melting, is compelled to leave its original position and seek a new one, which it of course finds only

below the snow-line on the lower slopes of the mountain, and especially in the valleys, where however being exposed to the influence of a warmer air, it ultimately melts and flows away as water. The descent of masses of snow from their original positions sometimes happens suddenly in *avalanches*, but it is usually very gradual in the form of *glaciers*.

Thus we must discriminate between two distinct parts of the ice-fields; that is, first, the snow which originally fell —called *firn* in Switzerland—above the snow-line, covering the slopes of the peaks as far as it can hang on to them, and filling up the upper wide kettle-shaped ends of the valleys forming widely extended fields of snow or *firnmeere*. Secondly, the glaciers, called in the Tyrol *firner*, which as prolongations of the snow-fields often extend to a distance of from 4,000 to 5,000 feet below the snow-line, and in which the loose snow of the snow-fields is again found changed into transparent solid ice. Hence the name *glacier*, which is derived from the Latin, *glacies*; French, *glace*, *glacier*.

The outward appearance of glaciers is very characteristically described by comparing them with Goethe to currents of ice. They generally stretch from the snow-fields along the depth of the valleys, filling them throughout their entire breadth, and often to a considerable height. They thus follow all the curvatures, windings, contractions, and enlargements of the valley. Two glaciers frequently meet, the valleys of which unite. The two glacial currents then join in one common principal current, filling up the valley common to them both. In some places these ice-currents present a tolerably level and coherent surface, but they are usually traversed by *crevasses*, and both over the surface and through the crevasses countless small and large water rills ripple, which carry off the water formed by the melting of the ice. United, *and forming* a stream, they burst, through a vaulted and

clear blue gateway of ice, out at the lower end of the larger glacier.

On the surface of the ice there is a large quantity of blocks of stone, and of rocky *débris*, which at the lower end of the glacier are heaped up and form immense walls; these are called the *lateral* and *terminal moraine* of the glacier. Other heaps of rock, the *central moraine*, stretch along the surface of the glacier in the direction of its

Fig. 13.

length, forming long regular dark lines. These always start from the places where two glacier streams coincide and unite. The central moraines are in such places to be regarded as the continuations of the united lateral moraines of the two glaciers.

The formation of the central moraine is well represented in the view above given of the Unteraar Glacier. Fig. 13.

In the background are seen the two glacier currents emerging from different valleys; on the right from the Schreckhorn, and on the left from the Finsteraarhorn. From the place where they unite the rocky wall occupying the middle of the picture descends, constituting the central moraine. On the left are seen individual large masses of rock resting on pillars of ice, which are known as glacier tables.

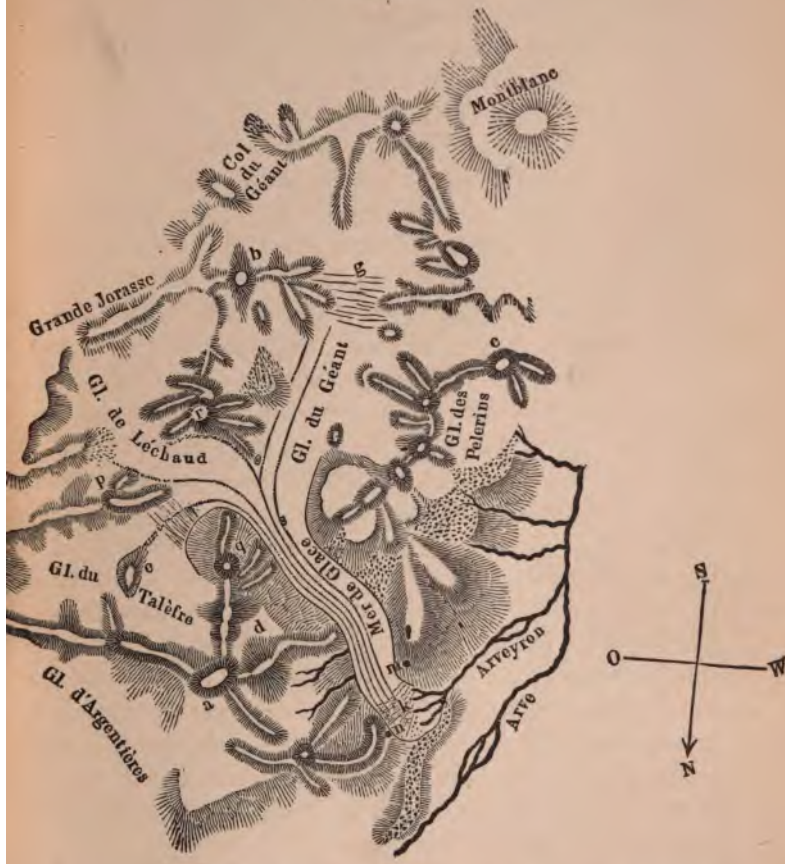
To exemplify these circumstances still further, I lay before you in Fig. 14 a map of the *Mer de Glace* of Chamouni, copied from that of Forbes.

The *Mer de Glace* in size is well known as the largest glacier in Switzerland, although in length it is exceeded by the Aletsch Glacier. It is formed from the snow-fields that cover the heights directly north of Mont Blanc, several of which, as the Grande Jorasse, the Aiguille Verte (a, Figs. 14 and 15), the Aiguille du Géant (b), Aiguille du Midi (c), and the Aiguille du Dru (d), are only 2,000 to 3,000 feet below that king of the European mountains. The snow-fields which lie on the slopes and in the basins between these mountains collect in three principal currents, the Glacier du Géant, Glacier de Léchaud, and Glacier du Talèfre, which, ultimately united as represented in the map, form the Mer de Glace; this stretches as an ice current 2,600 to 3,000 feet in breadth down into the valley of Chamouni, where a powerful stream, the Arveyron, bursts from its lower end at k, and plunges into the Arve. The lowest precipice of the Mer de Glace, which is visible from the valley of Chamouni, and forms a large cascade of ice, is commonly called Glacier des Bois, from a small village which lies below.

Most of the visitors at Chamouni only set foot on the lowest part of the Mer de Glace from the inn at the Montanvert, and when they are free from giddiness cross the glacier at this place to the little house on the oppo-

site side, the Chapeau (n). Although, as the map shows, only a comparatively very small portion of the glacier is thus seen and crossed, this way shows sufficiently the

Fig. 14.



magnificent scenes, and also the difficulties of a glacier excursion. Bolder wanderers march upwards along the glacier to the Jardin, a rocky cliff clothed with som-

vegetation, which divides the glacial current of the Glacier du Talèfre into two branches; and bolder still they ascend yet higher, to the Col du Géant (11,000 feet above the sea), and down the Italian side to the valley of Aosta.

The surface of the Mer de Glace shows four of the rocky walls which we have designated as medial moraines. The first, nearest the east side of the glacier, is formed where the two arms of the Glacier du Talèfre unite at the lower end of the Jardin; the second proceeds from the union of the glacier in question with the Glacier de Léchaud; the third, from the union of the last with the Glacier du Géant; and the fourth, finally, from the top of the rock ledge which stretches from the Aiguille du Géant towards the cascade (g) of the Glacier du Géant.

To give you an idea of the slope and the fall of the glacier, I have given in Fig. 15 a longitudinal section of it according to the levels and measurements taken by Forbes, with the view of the right bank of the glacier. The letters stand for the same objects as in Fig. 14; p is the Aiguille de Léchaud, q the Aiguille Noire, r the Mont Tacul, f is the Col du Géant, the lowest point in the high wall of rock that surrounds the upper end of the snow-fields which feed the Mer de Glace. The base line corresponds to a length of a little more than nine miles: on the right the heights above the sea are given in feet. The drawing shows very distinctly how small in most places is the fall of the glacier. Only an approximate estimate could be made of the depth, for hitherto nothing certain has been made out in reference to it. But that it is very deep is obvious from the following individual and accidental observations.

At the end of a vertical rock wall of the Tacul, the edge of the Glacier du Géant is pushed forth, forming an ice wall 140 feet in height. This would give the depth of one of the upper arms of the glacier at the edge. In

the middle and after the union of the three glaciers the depth must be far greater. Somewhat below the junction Tyndall and Hirst sounded a moulin, that is a cavity through which the surface glacier waters escape, to a depth of 160 feet; the guides alleged that they had sounded a similar aperture to a depth of 350 feet, and had found no bottom. From the usually deep trough shaped or gorge-like form of the bottom of the valleys, which is constructed solely of rock walls, it seems improbable that for a breadth of 3,000 feet the mean depth should only be 350 feet; moreover, from the manner in which ice moves, there must necessarily be a very thick coherent mass beneath the crevassed part.

To render these magnitudes more intelligible by reference to more familiar objects, imagine the valley of Heidelberg filled with ice up to the Molkencur, or higher, so that the whole town, with all its steeples and the castle, is buried deeply beneath it; if, further, you imagine this mass of ice, gradually extending in height, continued from the mouth of the valley up to Neckargemünd, that would *about correspond* to the lower

Fig. 15.

united ice current of the Mer de Glace. Or, instead of the Rhine and the Nahe at Bingen, suppose two ice currents uniting which fill the Rhine valley to its upper border as far as we can see from the river, and then the united currents stretching downwards to beyond Asmannshausen and Burg Rheinstein; such a current would also about correspond to the size of the Mer de Glace.

Fig. 16, which is a view of the magnificent Gorner

Fig. 16.

Glacier seen from below, also gives an idea of the size of the masses of ice of the larger glaciers.

The surface of most glaciers is dirty, from the numerous pebbles and sand which lie upon it, and which are heaped together the more the ice under them and among them melts away. The ice of the surface has been partially destroyed and rendered crumbly. In the depths of the crevasses ice is seen of a purity and clearness with which

nothing that we are acquainted with on the plains can be compared. From its purity it shows a splendid blue, like that of the sky, only with a greenish hue. Crevasses in which pure ice is visible in the interior occur of all sizes; in the beginning they form slight cracks in which a knife can scarcely be inserted; becoming gradually enlarged to chasms, hundreds, or even thousands, of feet in length, and twenty, fifty, and as much as a hundred feet in breadth, while some of them are immeasurably deep. Their vertical dark blue walls of crystal ice, glistening with moisture from the trickling water, form one of the most splendid spectacles which nature can present to us; but, at the same time, a spectacle strongly impregnated with the excitement of danger, and only enjoyable by the traveller who feels perfectly free from the slightest tendency to giddiness. The tourist must know how, with the aid of well-nailed shoes and a pointed Alpenstock, to stand even on slippery ice, and at the edge of a vertical precipice the foot of which is lost in the darkness of night, and at an unknown depth. Such crevasses cannot always be evaded in crossing the glacier; at the lower part of the Mer de Glace, for instance, where it is usually crossed by travellers, we are compelled to travel along some extent of precipitous banks of ice, which are occasionally only four to six feet in breadth, and on each side of which is such a blue abyss. Many a traveller, who has crept along the steep rocky slopes without fear, there feels his heart sink, and cannot turn his eyes from the yawning chasm, for he must first carefully select every step for his feet. And yet these blue chasms, which lie open and exposed in the daylight, are by no means the worst dangers of the glacier; though indeed we are so organised that a danger which we perceive, and which therefore we can safely avoid, frightens us far more than one which we know to exist, but which

is veiled from our eyes. So also it is with glacier chasms. In the lower part of the glacier they yawn before us, threatening death and destruction, and lead us, timidly collecting all our presence of mind, to shrink from them; thus accidents seldom occur. On the upper part of the glacier, on the contrary, the surface is covered with snow; this, when it falls thickly, soon arches over the narrower crevasses of a breadth of from four to eight feet, and forms bridges which quite conceal the crevasse, so that the traveller only sees a beautiful plane snow surface before him. If the snow bridges are thick enough, they will support a man; but they are not always so, and these are the places where men, and even chamois, are so often lost. These dangers may readily be guarded against if two or three men are roped together at intervals of ten or twelve feet. If then one of them falls into a crevasse, the two others can hold him, and draw him out again.

In some places the crevasses may be entered, especially at the lower end of a glacier. In the well-known glaciers of Grindelwald, Rosenlaui, and other places, this is facilitated by cutting steps and arranging wooden planks. Then anyone who does not fear the perpetually trickling water may explore these crevasses, and admire the wonderfully transparent and pure crystal walls of these caverns. The beautiful blue colour which they exhibit is the natural colour of perfectly pure water; liquid water as well as ice is blue, though to an extremely small extent, so that the colour is only visible in layers of from ten to twelve feet in thickness. The water of the Lake of Geneva and of the Lago di Garda exhibits the same splendid colour as ice.

The glaciers are not everywhere crevassed; in places where the ice meets with an obstacle, and in the middle of great glacier currents the motion of which is uniform, the surface is perfectly coherent.

Fig. 17 represents one of the more level parts of the Mer de Glace at the Montanvert, the little house of which is seen in the background. The Gries Glacier, where it forms the height of the pass from the Upper Rhone valley to the Tosa valley, may even be crossed on

Fig. 17.

horseback. We find the greatest disturbance of the surface of the glacier in those places where it passes from a slightly inclined part of its bed to one where the slope is steeper. The ice is there torn in all directions into a quantity of detached blocks, which by melting

are usually changed into wonderfully shaped sharp ridges and pyramids, and from time to time fall into the interjacent crevasses with a loud rumbling noise. Seen from a distance such a place appears like a wild frozen waterfall, and is therefore called a cascade; such a cascade is seen in the Glacier du Talèfre at l, another is seen in the Glacier du Géant at g, Fig. 14, while a third forms the lower end of the Mer de Glace. The latter, already mentioned as the Glacier des Bois, which rises directly from the trough of the valley at Chamouni to a height of 1,700 feet, the height of the Königstuhl at Heidelberg, affords at all times a chief object of admiration to the Chamouni tourist. Fig. 18 represents a view of its fantastically rent blocks of ice.

We have hitherto compared the glacier with a current as regards its outer form and appearance. This similarity, however, is not merely an external one: the ice of the glacier does, indeed, move forwards like the water of a stream, only more slowly. That this must be the case follows from the considerations by which I have endeavoured to explain the origin of a glacier. For as the ice is being constantly diminished at the lower end by melting, it would entirely disappear if fresh ice did not continually press forward from above, which, again, is made up by the snowfalls on the mountain tops.

But by careful ocular observation we may convince ourselves that the glacier does actually move. For the inhabitants of the valleys, who have the glaciers constantly before their eyes, often cross them, and in so doing make use of the larger blocks of stone as sign posts—detect this motion by the fact that their guide posts gradually descend in the course of each year. And as the yearly displacement of the lower half of the Mer de Glace at Chamouni amounts to no less than from 400 to 600 feet, you can readily conceive that such displacements must

ultimately be observed, notwithstanding the slow rate at which they take place, and in spite of the chaotic confusion of crevasses and rocks which the glacier exhibits.

Besides rocks and stones, other objects which have accidentally alighted upon the glacier are dragged along.

Fig. 18.

In 1788 the celebrated Genevese Saussure, together with his son and a company of guides and porters, spent sixteen days on the Col du Géant. On descending the rocks at the side of the cascade of the Glacier du Géant, they

left behind them a wooden ladder. This was at the foot of the Aiguille Noire, where the fourth band of the Mer de Glace begins; this line thus marks at the same time the direction in which ice travels from this point. In the year 1832, that is, forty-four years after, fragments of this ladder were found by Forbes and other travellers not far below the junction of the three glaciers of the Mer de Glace, in the same line (at s, Fig. 19), from

Fig. 19.

which it results that these parts of the glacier must on the average have each year descended 375 feet.

In the year 1827 Hugi had built a hut on the central moraine of the Unteraar Glacier for the purpose of making observations; the exact position of this hut was

determined by himself and afterwards by Agassiz, and they found that each year it had moved downwards. Fourteen years later, in the year 1841, it was 4,884 feet lower, so that every year it had on the average moved through 349 feet. Agassiz afterwards found that his own hut, which he had erected on the same glacier, had moved to a somewhat smaller extent. For these observations a long time was necessary. But if the motion of the glacier be observed by means of accurate measuring instruments, such as theodolites, it is not necessary to wait for years to observe that ice moves—a single day is sufficient.

Such observations have in recent times been made by several observers, especially by Forbes and by Tyndall. They show that in summer the middle of the Mer de Glace moves through twenty inches a day, while towards the lower terminal cascade the motion amounts to as much as thirty-five inches in a day. In winter the velocity is only about half as great. At the edges and in the lower layers of the glacier, as in a flow of water, it is considerably smaller than in the centre of the surface.

The upper sources of the Mer de Glace also have a slower motion, the Glacier du Géant thirteen inches a day, and the Glacier du Léchaud nine inches and a half. In different glaciers the velocity is in general very various, according to the size, the inclination, the amount of snow-fall, and other circumstances.

Such an enormous mass of ice thus gradually and gently moves on, imperceptibly to the casual observer, about an inch an hour—the ice of the Col du Géant will take 120 years before it reaches the lower end of the Mer de Glace—but it moves forward with uncontrollable force, before which any obstacles that man could oppose to it yield like straws, and the traces

of which are distinctly seen even on the granite walls of the valley. If, after a series of wet seasons, and an abundant fall of snow on the heights, the base of a glacier advances, not merely does it crush dwelling houses, and break the trunks of powerful trees, but the glacier pushes before it the boulder walls which form its terminal moraine without seeming to experience any resistance. A truly magnificent spectacle is this motion, so gentle, and so continuous, and yet so powerful and so irresistible.

I will mention here that from the way in which the glacier moves we can easily infer in what places and in what directions crevasses will be formed. For as all layers of the glacier do not advance with equal velocity, some points remain behind others: for instance, the edges as compared with the middle. Thus if we observe the distance from a given point at the edge to a given point of the middle, both of which were originally in the same line, but the latter of which afterwards descended more rapidly, we shall find that this distance continually increases; and since the ice cannot expand to an extent corresponding to the increasing distance, it breaks up and forms crevasses, as seen along the edge of the glacier in Fig. 20, which represents the Gorner Glacier at Zermatt. It would lead me too far if I were here to attempt to give a detailed explanation of the formation of the more regular system of crevasses, as they occur in certain parts of all glaciers; it may be sufficient to mention that the conclusions deducible from the considerations above stated are fully borne out by observation.

I will only draw attention to one point—what extremely small displacements are sufficient to cause ice to form hundreds of crevasses. The section of the Mer de Glace (Fig. 21, at g, c, h) shows places where a scarcely

perceptible change in the inclination of the surface of the ice occurs of from two to four degrees. This is sufficient to produce a system of cross crevasses on the surface. Tyndall more especially has urged and confirmed by observation and measurements, that the mass of ice of the glacier does not give way in the smallest

FIG. 20.

degree to extension, but when subjected to a pull is invariably torn asunder.

The distribution of the boulders, too, on the surface of the glacier is readily explained when we take their motion into account. These boulders are fragments of the mountains between which the glacier flows. Detached

Fig. 21.

partly by the weathering of the stone, and partly by the freezing of water in its crevices, they fall, and for the most part on the edge of the mass of ice. There they either remain lying on the surface, or if they have originally burrowed in the snow, they ultimately reappear in consequence of the melting of the superficial layers of ice and snow, and they accumulate especially at the lower end of the glacier, where more of the ice between them has been melted. The blocks which are gradually borne down to the lower end of the glacier are sometimes quite colossal in size. Solid rocky masses of this kind are met with in the lateral and terminal moraines, which are as large as a two-storied house.

The masses of stone move in lines which are always nearly parallel to each other and to the longitudinal direction of the glacier. Those, therefore, that are already in the middle remain in the middle, and those that lie on the edge remain at the edge. These latter are the more numerous, for during the entire course of the glacier, fresh boulders are con-

stantly falling on the edge, but cannot fall on the middle. Thus are formed on the edge of the mass of ice the lateral moraines, the boulders of which partly move along with the ice, partly glide over its surface, and partly rest on the solid rocky base near the ice. But when two glacier streams unite, their coinciding lateral moraines come to lie upon the centre of the united ice-stream, and then move forward as central moraines parallel to each other and to the banks of the stream, and they show, as far as the lower end, the boundary-line of the ice which originally belonged to one or the other of the arms of the glacier. They are very remarkable as displaying in what regular parallel bands the adjacent parts of the ice-stream glide downwards. A glance at the map of the Mer de Glace, and its four central moraines, exhibits this very distinctly.

On the Glacier du Géant and its continuation in the Mer de Glace, the stones on the surface of the ice delineate, in alternately grayer and whiter bands, a kind of yearly rings which were first observed by Forbes. For since in the cascade at g, Fig. 21, more ice slides down in summer than in winter, the surface of the ice below the cascade forms a series of terraces as seen in the drawing, and as those slopes of the terraces which have a northern aspect melt less than their upper plane surfaces, the former exhibit purer ice than the latter. This, according to Tyndall, is the probable origin of these dirt bands. At first they run pretty much across the glacier, but as afterwards their centre moves somewhat more rapidly than the ends, they acquire farther down a curved shape, as represented in the map, Fig. 19. By their curvature they thus show to the observer with what varying velocity ice advances in the different parts of its course.

A very peculiar part is played by certain stones which

are imbedded in the lower surface of the mass of ice, and which have partly fallen there through crevasses, and may partly have been detached from the bottom of the valley. For these stones are gradually pushed with the ice along the base of the valley, and at the same time are pressed against this base by the enormous weight of the superincumbent ice. Both, the stones imbedded in the ice, as well as the rocky base, are equally hard, but by their friction against each other they are ground to powder with a power compared to which any human exertion of force is infinitely small. The product of this friction is an extremely fine powder which, swept away by water, appears lower down in the glacier brook, imparting to it a whitish or yellowish muddy appearance.

The rocks of the trough of the valley, on the contrary, on which the glacier exerts year by year its grinding power, are polished as if in an enormous polishing machine. They remain as rounded, smoothly polished masses, in which are occasional scratches produced by individual harder stones. Thus we see them appear at the edge of existing glaciers, when after a series of dry and hot seasons the glaciers have somewhat receded. But we find such polished rocks as remains of gigantic ancient glaciers to a far greater extent in the lower parts of many Alpine valleys. In the valley of the Aar more especially, as far down as Meyringen, the rock-walls polished to a considerable height are very characteristic. There also we find the celebrated polished plates, over which the way passes, and which are so smooth that furrows have had to be hewn into them and rails erected to enable men and animals to traverse them in safety.

The former enormous extent of glaciers is recognised by ancient moraine-dykes, and by transported blocks of stone, as well as by these polished rocks. The blocks of stone which have been carried away by the glacier are

distinguished from those which water has rolled down, by their enormous magnitude, by the perfect retention of all their edges which are not at all rounded off, and finally by their being deposited on the glacier in exactly the same order in which the rocks of which they formed part stand in the mountain ridge; while the stones which currents of water carry along are completely mixed together.

From these indications, geologists have been able to prove that the glaciers of Chamouni, of Monte Rosa, of the St. Gotthard, and the Bernese Alps, formerly penetrated through the valley of the Arve, the Rhone, the Aare, and the Rhine to the more level part of Switzerland and the Jura, where they have deposited their boulders at a height of more than a thousand feet above the present level of the Lake of Neufchatel. Similar traces of ancient glaciers are found upon the mountains of the British Islands, and upon the Scandinavian Peninsula.

The drift-ice too of the Arctic Sea is glacier ice; it is pushed down into the sea by the glaciers of Greenland, becomes detached from the rest of the glacier, and floats away. In Switzerland we find a similar formation of drift-ice, though on a far smaller scale, in the little Marjelen See, into which part of the ice of the great Aletsch Glacier pushes down. Blocks of stone which lie in drift-ice may make long voyages over the sea. The vast number of blocks of granite which are scattered on the North German plains, and whose granite belongs to the Scandinavian mountains, has been transported by drift-ice at the time when the European glaciers had such an enormous extent.

I must unfortunately content myself with these few references to the ancient history of glaciers, and revert now to the processes at present at work in them.

From the facts which I have brought before you it results that the ice of a glacier flows slowly like the current of a very viscous substance, such for instance as honey, tar, or thick magma of clay. The mass of ice does not merely flow along the ground like a solid which glides over a precipice, but it bends and twists in itself; and although even while doing this it moves along the base of the valley, yet the parts which are in contact with the bottom and the sides of the valley are perceptibly retarded by the powerful friction; the middle of the surface of the glacier, which is most distant both from the bottom and the sides, moving most rapidly. Rendu, a Savoyard priest, and the celebrated natural philosopher Forbes, were the first to suggest the similarity of a glacier with a current of a viscous substance.

Now you will perhaps enquire with astonishment how it is possible that ice, which is the most brittle and fragile of substances, can flow in the glacier like a viscous mass; and you may perhaps be disposed to regard this as one of the wildest and most improbable statements that have ever been made by philosophers. I will at once admit that philosophers themselves were not a little perplexed by these results of their investigations. But the facts were there, and could not be got rid of. How this mode of motion originated was for a long time quite enigmatical, the more so since the numerous crevasses in glaciers were a sufficient indication of the well-known brittleness of ice; and as Tyndall correctly remarked, this constituted an essential difference between a stream of ice and the flow of lava, of tar, of honey, or of a current of mud.

The solution of this strange problem was found, as is so often the case in the natural sciences, in apparently recondite investigations into the nature of heat, which form one of the most important conquests of modern

physics, and which constitute what is known as the *mechanical theory of heat.* Among a great number of deductions as to the relations of the diverse natural forces to each other, the principles of the mechanical theory of heat lead to certain conclusions as to the dependence of the freezing-point of water on the pressure to which ice and water are exposed.

Everyone knows that we determine that one fixed point of our thermometer scale which we call the *freezing-point* or *zero*, by placing the thermometer in a mixture of pure water and ice. Water, at any rate when in contact with ice, cannot be cooled below zero without itself being converted into ice; ice cannot be heated above the freezing-point without melting. Ice and water can exist in each other's presence at only one temperature, the temperature of zero.

Now, if we attempt to heat such a mixture by a flame beneath it, the ice melts, but the temperature of the mixture is never raised above that of 0° so long as some of the ice remains unmelted. The heat imparted changes ice at zero into water at zero, but the thermometer indicates no increase of temperature. Hence physicists say that heat has become *latent*, and that water contains a certain quantity of latent heat beyond that of ice at the same temperature.

On the other hand, when we withdraw more heat from the mixture of ice and water, the water gradually freezes; but as long as there is still liquid water, the temperature remains at zero. Water at 0° has given up its latent heat, and has become changed into ice at 0°.

Now a glacier is a mass of ice which is everywhere interpenetrated by water, and its internal temperature is therefore everywhere that of the freezing-point. The deeper layers, even of the fields of névé, appear on the heights which occur in our Alpine chain to have every

where the same temperature. For, though the freshly-fallen snow of these heights is, for the most part, at a lower temperature than that of 0°, the first hours of warm sunshine melt its surface and form water, which trickles into the deeper colder layers, and there freezes, until it has throughout been brought to the temperature of the freezing-point. This temperature then remains unchanged. For, though by the sun's rays the surface of the ice may be melted, it cannot be raised above zero, and the cold of winter penetrates as little into the badly-conducting masses of snow and ice as it does into our cellars. Thus the interior of the masses of névé, as well as of the glacier, remains permanently at the melting-point.

But the temperature at which water freezes may be altered by strong pressure. This was first deduced from the mechanical theory of heat by James Thomson of Belfast, and almost simultaneously by Clausius of Zürich; and, indeed, the amount of this change may be correctly predicted from the same reasoning. For each increase of a pressure of one atmosphere the freezing-point is lowered by the $\frac{1}{115}$th part of a degree Centigrade. The brother of the former, Sir W. Thomson, the celebrated Glasgow physicist, made an experimental confirmation of this theoretical deduction by compressing in a suitable vessel a mixture of ice and snow. This mixture became colder and colder as the pressure was increased, and to the extent required by the mechanical theory.

Now, if a mixture of ice and water becomes colder when it is subjected to increased pressure without the withdrawal of heat, this can only be effected by some free heat becoming latent; that is, some ice in the mixture must melt and be converted into water. In this is found the reason why mechanical pressure can influence the freezing-point. You know that ice occu-

pies more space than the water from which it is formed. When water freezes in closed vessels, it can burst not only glass vessels, but even iron shells. Inasmuch, therefore, as in the compressed mixture of ice and water some of the ice melts and is converted into water, the *volume* of the mass diminishes, and the mass can yield *more* to the pressure upon it than it could have done without such an alteration of the freezing-point. Pressure furthers in this case, as is usual in the interaction of various natural forces, the occurrence of a change, that is fusion, which is favourable to the development of its own activity.

In Sir W. Thomson's experiments, water and ice were confined in a closed vessel, from which nothing could escape. The case is somewhat different when, as with glaciers, the water disseminated in the compressed ice can escape through fissures. The ice is then compressed, but not the water which escapes. The compressed ice becomes colder in conformity with the lowering of its freezing-point by pressure; but the freezing-point of water which is not compressed is not lowered. Thus under these circumstances we have ice colder than 0° in contact with water at 0°. The consequence is that around the compressed ice water continually freezes and forms new ice, while on the other hand part of the compressed ice melts.

This occurs, for instance, when only two pieces of ice are pressed against each other. By the water which freezes at their surfaces of contact they are firmly joined into one coherent piece of ice. With powerful pressure, and the chilling therefore great, this is quickly effected; but even with a feeble pressure it takes place, if sufficient time be given. Faraday, who discovered this property, called it the *regelation of ice*; the explanation

of this phenomenon has been much controverted; I have detailed to you that which I consider most satisfactory.

This freezing together of two pieces of ice is very readily effected by pieces of any shape, which must not, however, be at a lower temperature than 0°, and the experiment succeeds best when the pieces are already in the act of melting.[1] They need only be strongly pressed together for a few minutes to make them adhere. The more plane are the surfaces in contact, the more complete is their union. But a very slight pressure is sufficient if the two pieces are left in contact for some time.[2]

This property of melting ice is also utilised by boys in making snow-balls and snow-men. It is well known that this only succeeds either when the snow is already melting, or at any rate is only so much lower than 0° that the warmth of the hand is sufficient to raise it to this temperature. Very cold snow is a dry loose powder which does not stick together.

The process which children carry out on a small scale in making snow-balls, takes place in glaciers on the very largest scale. The deeper layers of what was originally fine loose névé are compressed by the huge masses resting on them, often amounting to several hundred feet, and under this pressure they cohere with an ever firmer and closer structure. The freshly-fallen snow originally consisted of delicate microscopically fine ice spicules, united and forming delicate six-rayed, feathery stars of extreme beauty. As often as the upper layers of the snow-fields are exposed to the sun's rays, some of the snow melts; water permeates the mass, and on reaching

[1] In the Lecture a series of small cylinders of ice, which had been prepared by a method to be afterwards described, were pressed with their plane ends against each other, and thus a cylindrical bar of ice produced.

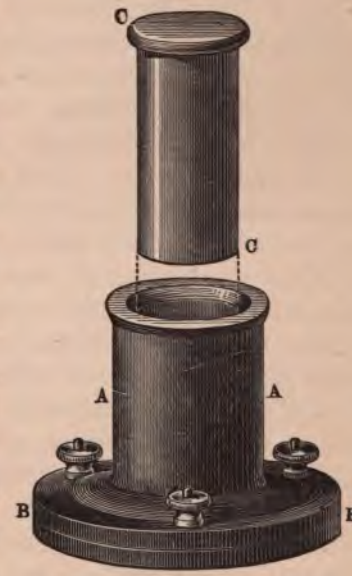
[2] Vide the additions at the end of this Lecture.

the lower layers of still colder snow, it again freezes; thus it is that the firn first becomes granular and acquires the temperature of the freezing-point. But as the weight of the superincumbent masses of snow continually increases by the firmer adherence of its individual granules, it ultimately changes into a dense and perfectly hard mass.

This transformation of snow into ice may be artificially effected by using a corresponding pressure.

We have here (Fig. 22) a cylindrical cast-iron vessel, A A; the base, B B, is held by three screws, and can be detached, so as to remove the cylinder of ice which is formed. After the vessel has lain for a while in ice-water, so as to reduce it to the temperature of 0°, it is packed full of snow, and then the cylindrical plug, C C, which fits the inner aperture, but moves in it with gentle friction, is forced in with the aid of an hydraulic press. The press used was such that the pressure to which the snow was exposed could be increased to fifty atmospheres. Of course the looser snow contracts to a very small volume under such a powerful pressure. The pressure is removed, the cylindrical plug taken out, the hollow again filled up with snow, and the process repeated until the entire form is filled with the mass of ice, which no longer gives way

Fig. 22.



to pressure. The compressed snow which I now take out, you will see, has been transformed into a hard, angular, and translucent cylinder of ice; and how hard it is, appears from the crash which ensues when I throw it to the ground. Just as the loose snow in the glaciers is pressed together to solid ice, so also in many places ready-formed irregular pieces of ice are joined and form clear and compact ice. This is most remarkable at the base of the glacier cascades. These are glacier falls where the upper part of the glacier ends at a steep rocky wall, and blocks of ice shoot down as avalanches over the edge of this wall. The heap of shattered blocks of ice which accumulate become joined at the foot of the rock-wall to a compact, dense mass, which then continues its way downwards as glacier. More frequent than such cascades, where the glacier-stream is quite dissevered, are places where the base of the valley has a steeper slope, as, for instance, the places in the Mer de Glace (Fig. 14), at g, of the Cascade of the Glacier du Géant, and at i and h of the great terminal cascade of the Glacier des Bois. The ice splits there into thousands of banks and cliffs, which then recombine towards the bottom of the steeper slope and form a coherent mass.

This also we may imitate in our ice-mould. Instead of the snow I take irregular pieces of ice, press them together; add new pieces of ice, press them again, and so on, until the mould is full. When the mass is taken out it forms a compact coherent cylinder of tolerably clear ice, which has a perfectly sharp edge, and is an accurate copy of the mould.

This experiment, which was first made by Tyndall, shows that a block of ice may be pressed into any mould just like a piece of wax. It might, perhaps, be thought that such a block had, by the pressure in the interior, been first reduced to powder so fine that it readily penetrated

every vice of the mould, and then that this powdered ice, like snow, was again combined by freezing. This suggests itself the more readily, since while the press is being worked a continual creaking and cracking is heard in the interior of the mould. Yet the mere aspect of the cylinders pressed from blocks of ice shows us that it has not been formed in this manner; for they are generally clearer than the ice which is produced from snow, and the individual larger pieces of ice which have been used to produce them are recognised, though they are somewhat changed and flattened. This is most beautiful when clear pieces of ice are laid in the form and the rest of the space stuffed full of snow. The cylinder is then seen to consist of alternate layers of clear and opaque ice, the former arising from the pieces of ice, and the latter from the snow; but here also the pieces of ice seem pressed into flat discs.

These observations teach, then, that ice need not be completely smashed to fit into the prescribed mould, but that it may give way without losing its coherence. This can be still more completely proved, and we can acquire a still better insight into the cause of the pliability of ice, if we press the ice between two plane wooden boards, instead of in the mould, into which we cannot see.

I place first an irregular cylindrical piece of natural ice, taken from the frozen surface of the river, with its two plane terminal surfaces between the plates of the press. If I begin to work, the block is broken by pressure; every crack which forms extends through the entire mass of the block; this splits into a heap of larger fragments, which again crack and are broken the more the press is worked. If the pressure is relaxed, all these fragments are, indeed, reunited by freezing, but the aspect of the whole indicates that the shape of the block has resulted less from pliability than from fracture.

and that the individual fragments have completely altered their mutual positions.

The case is quite different when one of the cylinders which we have formed from snow or ice is placed between the plates of the press. As the press is worked the creaking and cracking is heard, but it does not break; it gradually changes its shape, becomes lower and at the same time thicker; and only when it has been changed into a tolerably flat circular disc does it begin to give way at the edges and form cracks, like crevasses on a small scale. Fig. 23 shows the height and diameter of such a cylinder in its original condition; Fig. 24 represents its appearance after the action of the press.

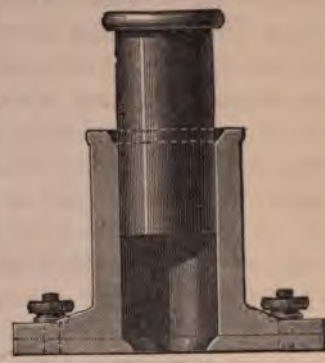
Fig. 23.

Fig. 24.

A still stronger proof of the pliability of ice is afforded when one of our cylinders is forced through a narrow aperture. With this view I place a base on the previously described mould, which has a conical perforation, the external aperture of which is only two-thirds the diameter of the cylindrical aperture of the form. Fig. 25 gives a section of the whole. If now I insert into this one of the compressed cylinders of ice, and force down the plug a, the ice is forced through the narrow aperture in

the base. It at first emerges as a solid cylinder of the same diameter as the aperture; but as the ice follows more rapidly in the centre than at the edges, the free terminal surface of the cylinder becomes curved, the end thickens, so that it could not be brought back through the aperture, and it ultimately splits off. Fig. 26 exhibits a series of shapes which have resulted in this manner.[1]

Fig. 25.



Here also the cracks in the emerging cylinder of ice exhibit a surprising similarity with the longitudinal rifts

Fig. 26.

which divide a glacier current where it presses through a narrow rocky pass into a wider valley.

In the cases which we have described we see the change in shape of the ice taking place before our eyes, whereby the block of ice retains its coherence without breaking into individual pieces. The brittle mass of ice seems rather to yield like a piece of wax.

A closer inspection of a clear cylinder of ice compressed

[1] In this experiment the lower temperature of the compressed ice sometimes extended so far through the iron form, that the water in the slit between the base plate and the cylinder froze and formed a thin sheet of ice, although the pieces of ice as well as the iron mould had previously laid in ice-water, and could not be colder than 0°.

from clear pieces of ice, while the pressure is being applied, shows us what takes place in the interior; for we then see an innumerable quantity of extremely fine radiating cracks shoot through it like a turbid cloud, which mostly disappear, though not completely, the moment the pressure is suspended. Such a compressed block is distinctly more opaque immediately after the experiment than it was before; and the turbidity arises, as may easily be observed by means of a lens, from a great number of whitish capillary lines crossing the interior of the mass of what is otherwise clear. These lines are the optical expression of extremely fine cracks[1] which interpenetrate the mass of the ice. Hence we may conclude that the compressed block is traversed by a great number of fine cracks and fissures, which render it pliable, that its particles become a little dispersed, and are therefore withdrawn from pressure, and that immediately afterwards the greater part of the fissures disappear, owing to their sides freezing. Only in those places in which the surfaces of the small displaced particles do not accurately fit to each other some fissured spaces remain open, and are discovered as white lines and surfaces by the reflection of the light.

These cracks and laminæ also become more perceptible when the ice—which, as I before mentioned, is below zero immediately after pressure has been applied—is again raised to this temperature and begins to melt. The cre-

[1] These cracks are probably quite empty and free from air, for they are also formed when perfectly clear and air-free pieces of ice are pressed in the form which has been previously filled with water, and where, therefore, no air could gain access to the pieces of ice. That such air-free crevices occur in glacier ice has been already demonstrated by Tyndall. When the compressed ice afterwards melts, these crevices fill up with water, no air being left. They are then, however, far less visible, and the whole block is therefore clearer. And just for this reason they could not originally have been filled with water.

vices then fill with water, and such ice then consists of a quantity of minute granules from the size of a pin's head to that of a pea, which are closely pushed into one another at the edges and projections, and in part have coalesced, while the narrow fissures between them are full of water. A block of ice thus formed of ice-granules adheres firmly together; but if particles be detached from its corners they are seen to consist of these angular granules. Glacier ice, when it begins to melt, is seen to possess the same structure, except that the pieces of which it consists are mostly larger than in artificial ice, attaining the size of a pigeon's egg.

Glacier ice and compressed ice are thus seen to be substances of a granular structure, in opposition to regularly crystallised ice, such as is formed on the surface of still water. We here meet with the same differences as between calcareous spar and marble, both of which consist of carbonate of lime; but while the former is in large, regular crystals, the latter is made up of irregularly agglomerated crystalline grains. In calcareous spar, as well as in crystallised ice, the cracks produced by inserting the point of a knife extend through the mass, while in granular ice a crack which arises in one of the bodies where it must yield does not necessarily spread beyond the limits of the granule.

Ice which has been compressed from snow, and has thus from the outset consisted of innumerable very fine crystalline needles, is seen to be particularly plastic. Yet in appearance it materially differs from glacier ice, for it is very opaque, owing to the great quantity of air which was originally enclosed in the flaky mass of snow, and which remains there as extremely minute bubbles. It can be made clearer by pressing a cylinder of such ice between wooden boards; the air-bubbles appear then on the top of the cylinder as a light foam. If the discs are

again broken, placed in the mould, and pressed into a cylinder, the air may gradually be more and more eliminated, and the ice be made clearer. No doubt in glaciers the originally whitish mass of névé is thus gradually transformed into the clear, transparent ice of the glacier.

Lastly, when streaked cylinders of ice formed from pieces of snow and ice are pressed into discs, they become finely streaked, for both their clear and their opaque layers are uniformly extended.

Ice thus striated occurs in numerous glaciers, and is no doubt caused, as Tyndall maintains, by snow falling between the blocks of ice; this mixture of snow and clear ice is again compressed in the subsequent path of the glacier, and gradually stretched by the motion of the mass: a process quite analogous to the artificial one which we have demonstrated.

Thus to the eye of the natural philosopher the glacier, with its wildly-heaped ice-blocks, its desolate, stony, and muddy surface, and its threatening crevasses, has become a majestic stream whose peaceful and regular flow has no parallel; which, according to fixed and definite laws, narrows, expands, is heaped up, or, broken and shattered, falls down precipitous heights. If we trace it beyond its termination we see its waters, uniting to a copious brook, burst through its icy gate and flow away. Such a brook, on emerging from the glacier, seems dirty and turbid enough, for it carries away as powder the stone which the glacier has ground. We are disenchanted at seeing the wondrously beautiful and transparent ice converted into such muddy water. But the water of the glacier streams is as pure and beautiful as the ice, though its beauty is for the moment concealed and invisible. We must search for these waters after they have passed through a lake in which they have deposited this powdered stone. The Lakes of Geneva, of Thun, of Lucerne,

of Constance, the Lago Maggiore, the Lake of Como, and the Lago di Garda are chiefly fed with glacier waters; their clearness and their wonderfully beautiful blue or blue-green colour are the delight of all travellers.

Yet, leaving aside the beauty of these waters, and considering only their utility, we shall have still more reason for admiration. The unsightly mud, which the glacier streams wash away, forms a highly fertile soil in the places where it is deposited; for its state of mechanical division is extremely fine, and it is moreover an utterly unexhausted virgin soil, rich in the mineral food of plants. The fruitful layers of fine loam which extend along the whole Rhine plain as far as Belgium, and are known as Loess, are nothing more than the dust of ancient glaciers.

Then, again, the irrigation of a district, which is effected by the snow-fields and glaciers of the mountains, is distinguished from that of other places by its comparatively greater abundancy, for the moist air which is driven over the cold mountain peaks deposits there most of the water it contains in the form of snow. In the second place, the snow melts most rapidly in summer, and thus the springs which flow from the snow-fields are most abundant in that season of the year in which they are most needed.

Thus we ultimately get to know the wild, dead ice-wastes from another point of view. From them trickles in thousands of rills, springs, and brooks the fructifying moisture which enables the industrious dwellers of the Alps to procure succulent vegetation and abundance of nourishment from the wild mountain slopes. On the comparatively small surface of the Alpine chain they produce the mighty streams, the Rhine, the Rhone, the Po, the Adige, the Inn, which for hundreds of miles form broad, rich river-valleys, extending through Europe to the German Ocean, the Mediterranean, the Adriatic, and the Black Sea. Let us call to mind how magnificently Goethe,

in 'Mahomet's Song,' has depicted the course of the rocky spring, from its origin beyond the clouds to its union with Father Ocean. It would be presumptuous after him to give such a picture in other than his own words:—

And along, in triumph rolling,
Names he gives to regions; cities
Grow amain beneath his feet.

On and ever on he rushes;
Spire and turret fiery crested,
Marble palaces, the creatures
Of his wealth, he leaves behind.

Pine-built houses bears the Atlas
On his giant shoulders. O'er his
Head a thousand pennons rustle,
Floating far upon the breezes,
Tokens of his majesty.

And so beareth he his brothers,
And his treasures, and his children,
To their primal sire expectant,
All his bosom throbbing, heaving
With a wild tumultuous joy.

THEODORE MARTIN'S *Translation.*

ADDITIONS.

THE theory of the regelation of ice has led to scientific discussions between Faraday and Tyndall on the one hand, and James and Sir W. Thomson on the other. In the text I have adopted the theory of the latter, and must now accordingly defend it.

Faraday's experiments show that a very slight pressure, not more than that produced by the capillarity of the layer of water between two pieces of ice, is sufficient to freeze them together. James Thomson observed that in Faraday's experiments, pressure which could freeze them together was not utterly wanting. I have satisfied myself by my own experiments that only very slight pressure is necessary. It must, however, be remembered, that the smaller the pressure the longer will be the time required to freeze the two pieces, and that then the junction will be very narrow and very fragile. Both these points are readily explicable on Thomson's theory. For under a feeble pressure the difference in temperature between ice and water will be very small, and the latent heat will only be slowly abstracted from the layers of water in contact with the pressed parts of the ice, so that a long time is necessary before they freeze. We must further take into account that we cannot in general consider that the two surfaces are quite in contact; under a feeble pressure which does not appreciably alter their shape, they will only touch in what are practically three points. A feeble total pressure on the pieces of ice concentrated on such narrow surfaces will always produce a tolerably great local pressure under the influence of which some ice will melt, and the water thus formed will freeze. But the bridge which joins them will never be otherwise than narrow.

Under stronger pressure, which may more completely alter the shape of the pieces of ice, and fit them against each other, and which will melt more of the surfaces that are first in contact, there will be a greater difference between the temperature of the ice and water, and the bridges will be more rapidly formed, and be of greater extent.

In order to show the slow action of the small differences of temperature which here come into play, I made the following experiments.

A glass flask with a drawn-out neck was half filled with water, which was boiled until all the air in the flask was driven out. The neck of the flask was then hermetically sealed. When cooled, the flask was void of air, and the water within it freed from the pressure of the atmosphere. As the water thus prepared can be cooled considerably below 0° C. before the first ice is formed, while when ice is in the flask it freezes at 0° C., the flask was in the first instance placed in a freezing mixture until the water was changed into ice. It was afterwards permitted to melt slowly in a place, the temperature of which was + 2° C., until the half of it was liquefied.

The flask thus half filled with water, having a disc of ice swimming upon it, was placed in a mixture of ice and water, being quite surrounded by the mixture. After an hour, the disc within the flask was frozen to the glass. By shaking the flask the disc was liberated, but it froze again. This occurred as often as the shaking was repeated.

The flask was permitted to remain for eight days in the mixture, which was kept throughout at a temperature of 0° C. During this time a number of very regular and sharply defined ice-crystals were formed, and augmented very slowly in size. This is perhaps the best method of obtaining beautifully formed crystals of ice.

While, therefore, the outer ice which had to support the pressure of the atmosphere slowly melted, the water within the flask, whose freezing-point, on account of a defect of pressure, was 0·0075° C. higher, deposited crystals of ice. The heat abstracted from the water in this operation had, moreover, to pass through the glass of the flask, which, together with the small difference of temperature, explains the slowness of the freezing process.

Now as the pressure of one atmosphere on a square millimetre amounts to about ten grammes, a piece of ice weighing ten grammes, which lies upon another and touches it in three places, the total surface of which is a square millimetre, will produce on these surfaces a pressure of an atmosphere. Ice will

therefore be formed more rapidly in the surrounding water than it was in the flask, where the side of the glass was interposed between the ice and the water. Even with a much smaller weight the same result will follow in the course of an hour. The broader the bridges become, owing to the freshly formed ice, the greater will be the surfaces over which the pressure exerted by the upper piece of ice is distributed, and the feebler it will become; so that with such feeble pressure the bridges can only slowly increase, and therefore they will be readily broken when we try to separate the pieces.

It cannot, moreover, be doubted, that in Faraday's experiments, in which two perforated discs of ice were placed in contact on a horizontal glass rod, so that gravity exerted no pressure, capillary attraction is sufficient to produce a pressure of some grammes between the plates, and the preceding discussions show that such a pressure, if adequate time be given, can form bridges between the plates.

If, on the other hand, two of the above-described cylinders of ice are powerfully pressed together by the hands, they adhere in a few minutes so firmly, that they can only be detached by the exertion of a considerable force, for which indeed that of the hands is sometimes inadequate.

In my experiments I found that the force and rapidity with which the pieces of ice united were so entirely proportional to the pressure, that I cannot but assign this as the actual and sufficient cause of their union.

In Faraday's explanation, according to which regelation is due to a contact action of ice and water, I find a theoretical difficulty. By the water freezing, a considerable quantity of latent heat must be set free, and it is not clear what becomes of this.

Finally, if ice in its change into water passes through an intermediate viscous condition, a mixture of ice and water which was kept for days at a temperature of 0° must ultimately assume this condition in its entire mass, provided its temperature was uniform throughout; this however is never the case.

As regards what is called the plasticity of ice, James Thomson has given an explanation of it in which the formation of cracks in the interior is not presupposed. No doubt when a mass of ice in different parts of the interior is exposed to different pressures,

a portion of the more powerfully compressed ice will melt; and the latent heat necessary for this will be supplied by the ice which is less strongly compressed, and by the water in contact with it. Thus ice would melt at the compressed places, and water would freeze in those which are not pressed: ice would thus be gradually transformed and yield to pressure. It is also clear that, owing to the very small conductivity for heat which ice possesses, a process of this kind must be extremely slow, if the compressed and colder layers of ice, as in glaciers, are at considerable distances from the less compressed ones, and from the water which furnishes the heat for melting.

To test this hypothesis, I placed in a cylindrical vessel, between two discs of ice of three inches in diameter, a smaller cylindrical piece of an inch in diameter. On the uppermost disc I placed a wooden disc, and this I loaded with a weight of twenty pounds. The section of the narrow piece was thus exposed to a pressure of more than an atmosphere. The whole vessel was packed between pieces of ice, and left for five days in a room, the temperature of which was a few degrees above the freezing-point. Under these circumstances the ice in the vessel, which was exposed to the pressure of the weight, should melt, and it might be expected that the narrow cylinder on which the pressure was most powerful should have been most melted. Some water was indeed formed in the vessel, but mostly at the expense of the larger discs at the top and bottom, which being nearest the outside mixture of ice and water could acquire heat through the sides of the vessel. A small welt, too, of ice, was formed round the surface of contact of the narrower with the lower broad piece, which showed that the water, which had been formed in consequence of the pressure, had again frozen in places in which the pressure ceased. Yet under these circumstances there was no appreciable alteration in the shape of the middle piece which was most compressed.

This experiment shows, that although changes in the shape of the pieces of ice must take place in the course of time in accordance with J. Thomson's explanation, by which the more strongly compressed parts melt, and new ice is formed at the places which are freed from pressure, these changes must be extremely slow when the thickness of the pieces of ice through which the heat

is conducted is at all considerable. Any marked change in shape by melting in a medium the temperature of which is everywhere 0°, could not occur without access of external heat, or from the uncompressed ice and water; and with the small differences in temperature which here come into play, and from the badly conducting power of ice, it must be extremely slow.

That on the other hand, especially in granular ice, the formation of cracks, and the displacement of the surfaces of those cracks, render such a change of form possible, is shown by the above-described experiments on pressure; and that in glacier ice changes of form thus occur, follows from the banded structure, and the granular aggregation which is manifest on melting, and also from the manner in which the layers change their position when moved, and so forth. Hence, I doubt not that Tyndall has discovered the essential and principal cause of the motion of glaciers, in referring it to the formation of cracks and to regelation.

I would at the same time observe that a quantity of heat, which is far from inconsiderable, must be produced by friction in the larger glaciers. It may be easily shown by calculation, that when a mass of firn moves from the Col du Géant to the source of the Arveyron, the heat due to the mechanical work would be sufficient to melt a fourteenth part of the mass. And as the friction must be greatest in those places that are most compressed, it will at any rate be sufficient to remove just those parts of the ice which offer most resistance to motion.

I will add in conclusion, that the above-described granular structure of ice is beautifully shown in polarised light. If a small clear piece is pressed in the iron mould, so as to form a disc of about five inches in thickness, this is sufficiently transparent for investigation. Viewed in the polarising apparatus, a great number of variously coloured small bands and rings are seen in the interior; and by the arrangement of their colours it is easy to recognise the limits of the ice-granules, which, heaped on one another in irregular order of their optical axes, constitute the plate. The appearance is essentially the same when the plate has just been taken out of the press, and the cracks appear in it as whitish lines, as afterwards when these crevices have been filled up in consequence of the ice beginning to melt.

In order to explain the continued coherence of the piece of ice during its change of form, it is to be observed that in general the cracks in the granular ice are only superficial, and do not extend throughout its entire mass. This is directly seen during the pressing of the ice. The crevices form and extend in different directions, like cracks produced by a heated wire in a glass tube. Ice possesses a certain degree of elasticity, as may be seen in a thin flexible plate. A fissured block of ice of this kind will be able to undergo a displacement at the two sides which form the crack, even when these continue to adhere in the unfissured part of the block. If then part of the fissure at first formed is closed by regelation, the fissure can extend in the opposite direction without the continuity of the block being at any time disturbed. It seems to me doubtful, too, whether in compressed ice and in glacier ice, which apparently consists of interlaced polyhedral granules, these granules, before any attempt is made to separate them, are completely detached from each other, and are not rather connected by ice bridges which readily give way; and whether these latter do not produce the comparatively firm coherence of the apparent heap of granules.

The properties of ice here described are interesting from a physical point of view, for they enable us to follow so closely the transition from a crystalline body to a granular one; and they give the causes of the alteration of its properties better than in any other well-known example. Most natural substances show no regular crystalline structure; our theoretical ideas refer almost exclusively to crystallised and perfectly elastic bodies. It is precisely in this relationship that the transition from fragile and elastic crystalline ice into plastic granular ice is so very instructive.

ON THE

INTERACTION OF NATURAL FORCES.

A LECTURE DELIVERED FEBRUARY 7, 1854, AT KÖNIGSBERG, IN PRUSSIA.

A NEW conquest of very general interest has been recently made by natural philosophy. In the following pages I will endeavour to give an idea of the nature of this conquest. It has reference to a new and universal natural law, which rules the action of natural forces in their mutual relations towards each other, and is as influential on our theoretic views of natural processes as it is important in their technical applications.

Among the practical arts which owe their progress to the development of the natural sciences, from the conclusion of the middle ages downwards, practical mechanics, aided by the mathematical science which bears the same name, was one of the most prominent. The character of the art was, at the time referred to, naturally very different from its present one. Surprised and stimulated by its own success, it thought no problem beyond its power, and immediately attacked some of the most difficult and complicated. Thus it was attempted to build automaton figures which should perform the functions of men and

animals. The marvel of the last century was Vaucanson's duck, which fed and digested its food; the flute-player of the same artist, which moved all its fingers correctly; the writing-boy of the elder, and the pianoforte-player of the younger Droz; which latter, when performing, followed its hands with its eyes, and at the conclusion of the piece bowed courteously to the audience. That men like those mentioned, whose talent might bear comparison with the most inventive heads of the present age, should spend so much time in the construction of these figures which we at present regard as the merest trifles, would be incomprehensible, if they had not hoped in solemn earnest to solve a great problem. The writing-boy of the elder Droz was publicly exhibited in Germany some years ago. Its wheelwork is so complicated, that no ordinary head would be sufficient to decipher its manner of action. When, however, we are informed that this boy and its constructor, being suspected of the black art, lay for a time in the Spanish Inquisition, and with difficulty obtained their freedom, we may infer that in those days even such a toy appeared great enough to excite doubts as to its natural origin. And though these artists may not have hoped to breathe into the creature of their ingenuity a soul gifted with moral completeness, still there were many who would be willing to dispense with the moral qualities of their servants, if at the same time their immoral qualities could also be got rid of; and to accept, instead of the mutability of flesh and bones, services which should combine the regularity of a machine with the durability of brass and steel.

The object, therefore, which the inventive genius of the past century placed before it with the fullest earnestness, and not as a piece of amusement merely, was boldly chosen, and was followed up with an expenditure of sagacity which has contributed not a little to enrich the mechanical

experience which a later time knew how to take advantage of. We no longer seek to build machines which shall fulfil the thousand services required of *one* man, but desire, on the contrary, that a machine shall perform *one* service, and shall occupy in doing it the place of a thousand men.

From these efforts to imitate living creatures, another idea, also by a misunderstanding, seems to have developed itself, and which, as it were, formed the new philosopher's stone of the seventeenth and eighteenth centuries. It was now the endeavour to construct a perpetual motion. Under this term was understood a machine, which, without being wound up, without consuming in the working of it falling water, wind, or any other natural force, should still continue in motion, the motive power being perpetually supplied by the machine itself. Beasts and human beings seemed to correspond to the idea of such an apparatus, for they moved themselves energetically and incessantly as long as they lived, and were never wound up; nobody set them in motion. A connexion between the supply of nourishment and the development of force did not make itself apparent. The nourishment seemed only necessary to grease, as it were, the wheelwork of the animal machine, to replace what was used up, and to renew the old. The development of force out of itself seemed to be the essential peculiarity, the real quintessence of organic life. If, therefore, men were to be constructed, a perpetual motion must first be found.

Another hope also seemed to take up incidentally the second place, which in our wiser age would certainly have claimed the first rank in the thoughts of men. The perpetual motion was to produce work inexhaustibly without corresponding consumption, that is to say, out of nothing. Work, however, is money. Here, therefore, the great

practical problem which the cunning heads of all centuries have followed in the most diverse ways, namely, to fabricate money out of nothing, invited solution. The similarity with the philosopher's stone sought by the ancient chemists was complete. That also was thought to contain the quintessence of organic life, and to be capable of producing gold.

The spur which drove men to inquiry was sharp, and the talent of some of the seekers must not be estimated as small. The nature of the problem was quite calculated to entice poring brains, to lead them round a circle for years, deceiving ever with new expectations which vanished upon nearer approach, and finally reducing these dupes of hope to open insanity. The phantom could not be grasped. It would be impossible to give a history of these efforts, as the clearer heads, among whom the elder Droz must be ranked, convinced themselves of the futility of their experiments, and were naturally not inclined to speak much about them. Bewildered intellects, however, proclaimed often enough that they had discovered the grand secret; and as the incorrectness of their proceedings was always speedily manifest, the matter fell into bad repute, and the opinion strengthened itself more and more that the problem was not capable of solution; one difficulty after another was brought under the dominion of mathematical mechanics, and finally a point was reached where it could be proved, that at least by the use of pure mechanical forces no perpetual motion could be generated.

We have here arrived at the idea of the driving force or power of a machine, and shall have much to do with it in future. I must therefore give an explanation of it. The idea of work is evidently transferred to machines by comparing their performances with those of men and animals, to replace which they were applied. We still

reckon the work of steam-engines according to horse-power. The value of manual labour is determined partly by the force which is expended in it (a strong labourer is valued more highly than a weak one), partly, however, by the skill which is brought into action. Skilled workmen are not to be had in any quantity at a moment's notice; they must have both talent and instruction, their education requires both time and trouble. A machine, on the contrary, which executes work skilfully, can always be multiplied to any extent; hence its skill has not the high value of human skill in domains where the latter cannot be supplied by machines. Thus the idea of the quantity of work in the case of machines has been limited to the consideration of the expenditure of force; this was the more important, as indeed most machines are constructed for the express purpose of exceeding, by the magnitude of their effects, the powers of men and animals. Hence, in a mechanical sense, the idea of work has become identical with that of the expenditure of force, and in this way I will apply it in the following pages.

How, then, can we measure this expenditure, and compare it in the case of different machines?

I must here conduct you a portion of the way—as short a portion as possible—over the uninviting field of mathematico-mechanical ideas, in order to bring you to a point of view from which a more rewarding prospect will open. And though the example which I will here choose, namely, that of a water-mill with iron hammer, appears to be tolerably romantic, still, alas! I must leave the dark forest valley, the foaming brook, the spark-emitting anvil, and the black Cyclops wholly out of sight, and beg a moment's attention for the less poetic side of the question, namely, the machinery. This is driven by a water-wheel, which in its turn is set in motion by the falling water. The axle of the water-wheel has at certain

places small projections, thumbs, which, during the rotation, lift the heavy hammer and permit it to fall again. The falling hammer belabours the mass of metal, which is introduced beneath it. The work therefore done by the machine consists, in this case, in the lifting of the hammer, to do which the gravity of the latter must be overcome. The expenditure of force will in the first place, other circumstances being equal, be proportional to the weight of the hammer; it will, for example, be double when the weight of the hammer is doubled. But the action of the hammer depends not upon its weight alone, but also upon the height from which it falls. If it falls through two feet, it will produce a greater effect than if it falls through only one foot. It is, however, clear that if the machine, with a certain expenditure of force, lifts the hammer a foot in height, the same amount of force must be expended to raise it a second foot in height. The work is therefore not only doubled when the weight of the hammer is increased twofold, but also when the space through which it falls is doubled. From this it is easy to see that the work must be measured by the product of the weight into the space through which it ascends. And in this way, indeed, we measure in mechanics. The unit of work is a foot-pound, that is, a pound weight raised to the height of one foot.

While the work in this case consists in the raising of the heavy hammer-head, the driving force which sets the latter in motion is generated by falling water. It is not necessary that the water should fall vertically, it can also flow in a moderately inclined bed; but it must always, where it has water-mills to set in motion, move from a higher to a lower position. Experiment and theory concur in teaching, that when a hammer of a hundredweight is to be raised one foot, to accomplish this at least a hundredweight of water must fall through the

space of one foot; or what is equivalent to this, two hundredweight must fall half a foot, or four hundredweight a quarter of a foot, &c. In short, if we multiply the weight of the falling water by the height through which it falls, and regard, as before, the product as the measure of the work, then the work performed by the machine in raising the hammer can, in the most favourable case, be only equal to the number of foot-pounds of water which have fallen in the same time. In practice, indeed, this ratio is by no means attained: a great portion of the work of the falling water escapes unused, inasmuch as part of the force is willingly sacrificed for the sake of obtaining greater speed.

I will further remark, that this relation remains unchanged whether the hammer is driven immediately by the axle of the wheel, or whether—by the intervention of wheelwork, endless screws, pulleys, ropes—the motion is transferred to the hammer. We may, indeed, by such arrangements succeed in raising a hammer of ten hundredweight, when by the first simple arrangement the elevation of a hammer of one hundredweight might alone be possible; but either this heavier hammer is raised to only one-tenth of the height, or tenfold the time is required to raise it to the same height; so that, however we may alter, by the interposition of machinery, the intensity of the acting force, still in a certain time, during which the mill-stream furnishes us with a definite quantity of water, a certain definite quantity of work, and no more, can be performed.

Our machinery, therefore, has in the first place done nothing more than make use of the gravity of the falling water in order to overpower the gravity of the hammer, and to raise the latter. When it has lifted the hammer to the necessary height, it again liberates it, and the hammer falls upon the metal mass which is pushed

beneath it. But why does the falling hammer here exercise a greater force than when it is permitted simply to press with its own weight on the mass of metal? Why is its power greater as the height from which it falls is increased, and the greater therefore the velocity of its fall? We find, in fact, that the work performed by the hammer is determined by its velocity. In other cases, also, the velocity of moving masses is a means of producing great effects. I only remind you of the destructive effects of musket-bullets, which in a state of rest are the most harmless things in the world. I remind you of the windmill, which derives its force from the moving air. It may appear surprising that motion, which we are accustomed to regard as a non-essential and transitory endowment of bodies, can produce such great effects. But the fact is, that motion appears to us, under ordinary circumstances, transitory, because the movement of all terrestrial bodies is resisted perpetually by other forces, friction, resistance of the air, &c., so that the motion is incessantly weakened and finally arrested. A body, however, which is opposed by no resisting force, when once set in motion moves onward eternally with undiminished velocity. Thus we know that the planetary bodies have moved without change through space for thousands of years. Only by resisting forces can motion be diminished or destroyed. A moving body, such as the hammer or the musket-ball, when it strikes against another, presses the latter together, or penetrates it, until the sum of the resisting forces presented by the body struck to pressure, or to the separation of its particles, is sufficiently great to destroy the motion of the hammer or of the bullet. The motion of a mass regarded as taking the place of working force is called the living force (*vis viva*) of the mass. The word 'living' has of course here no reference whatever to living beings, but is intended to

represent solely the force of the motion as distinguished from the state of unchanged rest—from the gravity of a motionless body, for example, which produces an incessant pressure against the surface which supports it, but does not produce any motion.

In the case before us, therefore, we had first power in the form of a falling mass of water, then in the form of a lifted hammer, and thirdly in the form of the living force of the falling hammer. We should transform the third form into the second, if we, for example, permitted the hammer to fall upon a highly elastic steel beam strong enough to resist the shock. The hammer would rebound, and in the most favourable case would reach a height equal to that from which it fell, but would never rise higher. In this way its mass would ascend; and at the moment when its highest point has been attained it would represent the same number of raised foot-pounds as before it fell, never a greater number; that is to say, living force can generate the same amount of work as that expended in its production. It is therefore equivalent to this quantity of work.

Our clocks are driven by means of sinking weights, and our watches by means of the tension of springs. A weight which lies on the ground, an elastic spring which is without tension, can produce no effects: to obtain such we must first raise the weight or impart tension to the spring, which is accomplished when we wind up our clocks and watches. The man who winds the clock or watch communicates to the weight or to the spring a certain amount of power, and exactly so much as is thus communicated is gradually given out again during the following twenty-four hours, the original force being thus slowly consumed to overcome the friction of the wheels and the resistance which the pendulum encounters from the air. The wheelwork of the clock therefore developes

no working force, which was not previously communicated to it, but simply distributes the force given to it uniformly over a longer time.

Into the chamber of an air-gun we squeeze, by means of a condensing air-pump, a great quantity of air. When we afterwards open the cock of the gun and admit the compressed air into the barrel, the ball is driven out of the latter with a force similar to that exerted by ignited powder. Now we may determine the work consumed in the pumping-in of the air, and the living force which, upon firing, is communicated to the ball, but we shall never find the latter greater than the former. The compressed air has generated no working force, but simply gives to the bullet that which has been previously communicated to it. And while we have pumped for perhaps a quarter of an hour to charge the gun, the force is expended in a few seconds when the bullet is discharged; but because the action is compressed into so short a time, a much greater velocity is imparted to the ball than would be possible to communicate to it by the unaided effort of the arm in throwing it.

From these examples you observe, and the mathematical theory has corroborated this for all purely mechanical, that is to say, for moving forces, that all our machinery and apparatus generate no force, but simply yield up the power communicated to them by natural forces,—falling water, moving wind, or by the muscles of men and animals. After this law had been established by the great mathematicians of the last century, a perpetual motion, which should make use solely of pure mechanical forces, such as gravity, elasticity, pressure of liquids and gases, could only be sought after by bewildered and ill-instructed people. But there are still other natural forces which are not reckoned among the purely moving forces,—heat, electricity, magnetism, light,

chemical forces, all of which nevertheless stand in manifold relation to mechanical processes. There is hardly a natural process to be found which is not accompanied by mechanical actions, or from which mechanical work may not be derived. Here the question of a perpetual motion remained open; the decision of this question marks the progress of modern physics, regarding which I promised to address you.

In the case of the air-gun, the work to be accomplished in the propulsion of the ball was given by the arm of the man who pumped in the air. In ordinary firearms, the condensed mass of air which propels the bullet is obtained in a totally different manner, namely, by the combustion of the powder. Gunpowder is transformed by combustion for the most part into gaseous products, which endeavour to occupy a much greater space than that previously taken up by the volume of the powder. Thus you see that, by the use of gunpowder, the work which the human arm must accomplish in the case of the air-gun is spared.

In the mightiest of our machines, the steam-engine, it is a strongly compressed aëriform body, water vapour, which, by its effort to expand, sets the machine in motion. Here also we do not condense the steam by means of an external mechanical force, but by communicating heat to a mass of water in a closed boiler, we change this water into steam, which, in consequence of the limits of the space, is developed under strong pressure. In this case, therefore, it is the heat communicated which generates the mechanical force. The heat thus necessary for the machine we might obtain in many ways: the ordinary method is to procure it from the combustion of coal.

Combustion is a chemical process. A particular constituent of our atmosphere, oxygen, possesses a strong force of attraction, or, as is said in chemistry, a strong affinity for the constituents of the combustible body,

which affinity, however, in most cases can only exert itself at high temperatures. As soon as a portion of the combustible body, for example the coal, is sufficiently heated, the carbon unites itself with great violence to the oxygen of the atmosphere and forms a peculiar gas, carbonic acid, the same that we see foaming from beer and champagne. By this combination light and heat are generated; heat is generally developed by any combination of two bodies of strong affinity for each other; and when the heat is intense enough, light appears. Hence in the steam-engine it is chemical processes and chemical forces which produce the astonishing work of these machines. In like manner the combustion of gunpowder is a chemical process, which in the barrel of the gun communicates living force to the bullet.

While now the steam-engine developes for us mechanical work out of heat, we can conversely generate heat by mechanical forces. Each impact, each act of friction does it. A skilful blacksmith can render an iron wedge red-hot by hammering. The axles of our carriages must be protected by careful greasing from ignition through friction. Even lately this property has been applied on a large scale. In some factories, where a surplus of water power is at hand, this surplus is applied to cause a strong iron plate to rotate rapidly upon another, so that they become strongly heated by the friction. The heat so obtained warms the room, and thus a stove without fuel is provided. Now could not the heat generated by the plates be applied to a small steam-engine, which in its turn should be able to keep the rubbing plates in motion? The perpetual motion would thus be at length found. This question might be asked, and could not be decided by the older mathematico-mechanical investigations. I will remark beforehand, that the general law which I will lay before you answers the question in the negative.

By a similar plan, however, a speculative American set some time ago the industrial world of Europe in excitement. The magneto-electric machines often made use of in the case of rheumatic disorders are well known to the public. By imparting a swift rotation to the magnet of such a machine we obtain powerful currents of electricity. If those be conducted through water, the latter will be resolved into its two components, oxygen and hydrogen. By the combustion of hydrogen, water is again generated. If this combustion takes place, not in atmospheric air, of which oxygen only constitutes a fifth part, but in pure oxygen, and if a bit of chalk be placed in the flame, the chalk will be raised to its white heat, and give us the sun-like Drummond's light. At the same time the flame developes a considerable quantity of heat. Our American proposed to utilise in this way the gases obtained from electrolytic decomposition, and asserted, that by the combustion a sufficient amount of heat was generated to keep a small steam-engine in action, which again drove his magneto-electric machine, decomposed the water, and thus continually prepared its own fuel. This would certainly have been the most splendid of all discoveries; a perpetual motion which, besides the force that kept it going, generated light like the sun, and warmed all around it. The matter was by no means badly thought out. Each practical step in the affair was known to be possible; but those who at that time were acquainted with the physical investigations which bear upon this subject, could have affirmed, on first hearing the report, that the matter was to be numbered among the numerous stories of the fable-rich America; and indeed a fable it remained.

It is not necessary to multiply examples further. You will infer from those given in what immediate connection heat, electricity, magnetism, light, and chemical affinity, stand with mechanical forces.

Starting from each of these different manifestations of natural forces, we can set every other in motion, for the most part not in one way merely, but in many ways. It is here as with the weaver's web,—

> Where a step stirs a thousand threads,
> The shuttles shoot from side to side,
> The fibres flow unseen,
> And one shock strikes a thousand combinations.

Now it is clear that if by any means we could succeed, as the above American professed to have done, by mechanical forces, in exciting chemical, electrical, or other natural processes, which, by any circuit whatever, and without altering permanently the active masses in the machine, could produce mechanical force in greater quantity than that at first applied, a portion of the work thus gained might be made use of to keep the machine in motion, while the rest of the work might be applied to any other purpose whatever. The problem was to find, in the complicated net of reciprocal actions, a track through chemical, electrical, magnetical, and thermic processes, back to mechanical actions, which might be followed with a final gain of mechanical work: thus would the perpetual motion be found.

But, warned by the futility of former experiments, the public had become wiser. On the whole, people did not seek much after combinations which promised to furnish a perpetual motion, but the question was inverted. It was no more asked, How can I make use of the known and unknown relations of natural forces so as to construct a perpetual motion? but it was asked, If a perpetual motion be impossible, what are the relations which must subsist between natural forces? Everything was gained by this inversion of the question. The relations of natural forces rendered necessary by the above assumption, might

be easily and completely stated. It was found that all known relations of forces harmonise with the consequences of that assumption, and a series of unknown relations were discovered at the same time, the correctness of which remained to be proved. If a single one of them could be proved false, then a perpetual motion would be possible.

The first who endeavoured to travel this way was a Frenchman named Carnot, in the year 1824. In spite of a too limited conception of his subject, and an incorrect view as to the nature of heat, which led him to some erroneous conclusions, his experiment was not quite unsuccessful. He discovered a law which now bears his name, and to which I will return further on.

His labours remained for a long time without notice, and it was not till eighteen years afterwards, that is in 1842, that different investigators in different countries, and independent of Carnot, laid hold of the same thought. The first who saw truly the general law here referred to, and expressed it correctly, was a German physician, J. R. Mayer of Heilbronn, in the year 1842. A little later, in 1843, a Dane named Colding presented a memoir to the Academy of Copenhagen, in which the same law found utterance, and some experiments were described for its further corroboration. In England, Joule began about the same time to make experiments having reference to the same subject. We often find, in the case of questions to the solution of which the development of science points, that several heads, quite independent of each other, generate exactly the same series of reflections.

I myself, without being acquainted with either Mayer or Colding, and having first made the acquaintance of Joule's experiments at the end of my investigation, followed the same path. I endeavoured to ascertain all the relations between the different natural processes, which followed from our regarding them from the above point of

Starting from each of these different manifestations of natural forces, we can set every other in motion, for the most part not in one way merely, but in many ways. It is here as with the weaver's web,—

> Where a step stirs a thousand threads,
> The shuttles shoot from side to side,
> The fibres flow unseen,
> And one shock strikes a thousand combinations.

Now it is clear that if by any means we could succeed, as the above American professed to have done, by mechanical forces, in exciting chemical, electrical, or other natural processes, which, by any circuit whatever, and without altering permanently the active masses in the machine, could produce mechanical force in greater quantity than that at first applied, a portion of the work thus gained might be made use of to keep the machine in motion, while the rest of the work might be applied to any other purpose whatever. The problem was to find, in the complicated net of reciprocal actions, a track through chemical, electrical, magnetical, and thermic processes, back to mechanical actions, which might be followed with a final gain of mechanical work: thus would the perpetual motion be found.

But, warned by the futility of former experiments, the public had become wiser. On the whole, people did not seek much after combinations which promised to furnish a perpetual motion, but the question was inverted. It was no more asked, How can I make use of the known and unknown relations of natural forces so as to construct a perpetual motion? but it was asked, If a perpetual motion be impossible, what are the relations which must subsist between natural forces? Everything was gained by this inversion of the question. The relations of natural forces rendered necessary by the above assumption, might

be easily and completely stated. It was found that all known relations of forces harmonise with the consequences of that assumption, and a series of unknown relations were discovered at the same time, the correctness of which remained to be proved. If a single one of them could be proved false, then a perpetual motion would be possible.

The first who endeavoured to travel this way was a Frenchman named Carnot, in the year 1824. In spite of a too limited conception of his subject, and an incorrect view as to the nature of heat, which led him to some erroneous conclusions, his experiment was not quite unsuccessful. He discovered a law which now bears his name, and to which I will return further on.

His labours remained for a long time without notice, and it was not till eighteen years afterwards, that is in 1842, that different investigators in different countries, and independent of Carnot, laid hold of the same thought. The first who saw truly the general law here referred to, and expressed it correctly, was a German physician, J. R. Mayer of Heilbronn, in the year 1842. A little later, in 1843, a Dane named Colding presented a memoir to the Academy of Copenhagen, in which the same law found utterance, and some experiments were described for its further corroboration. In England, Joule began about the same time to make experiments having reference to the same subject. We often find, in the case of questions to the solution of which the development of science points, that several heads, quite independent of each other, generate exactly the same series of reflections.

I myself, without being acquainted with either Mayer or Colding, and having first made the acquaintance of Joule's experiments at the end of my investigation, followed the same path. I endeavoured to ascertain all the relations between the different natural processes, which followed from our regarding them from the above point of

Starting from each of these different manifestations of natural forces, we can set every other in motion, for the most part not in one way merely, but in many ways. It is here as with the weaver's web,—

> Where a step stirs a thousand threads,
> The shuttles shoot from side to side,
> The fibres flow unseen,
> And one shock strikes a thousand combinations.

Now it is clear that if by any means we could succeed, as the above American professed to have done, by mechanical forces, in exciting chemical, electrical, or other natural processes, which, by any circuit whatever, and without altering permanently the active masses in the machine, could produce mechanical force in greater quantity than that at first applied, a portion of the work thus gained might be made use of to keep the machine in motion, while the rest of the work might be applied to any other purpose whatever. The problem was to find, in the complicated net of reciprocal actions, a track through chemical, electrical, magnetical, and thermic processes, back to mechanical actions, which might be followed with a final gain of mechanical work: thus would the perpetual motion be found.

But, warned by the futility of former experiments, the public had become wiser. On the whole, people did not seek much after combinations which promised to furnish a perpetual motion, but the question was inverted. It was no more asked, How can I make use of the known and unknown relations of natural forces so as to construct a perpetual motion? but it was asked, If a perpetual motion be impossible, what are the relations which must subsist between natural forces? Everything was gained by this inversion of the question. The relations of natural forces rendered necessary by the above assumption, might

be easily and completely stated. It was found that all known relations of forces harmonise with the consequences of that assumption, and a series of unknown relations were discovered at the same time, the correctness of which remained to be proved. If a single one of them could be proved false, then a perpetual motion would be possible.

The first who endeavoured to travel this way was a Frenchman named Carnot, in the year 1824. In spite of a too limited conception of his subject, and an incorrect view as to the nature of heat, which led him to some erroneous conclusions, his experiment was not quite unsuccessful. He discovered a law which now bears his name, and to which I will return further on.

His labours remained for a long time without notice, and it was not till eighteen years afterwards, that is in 1842, that different investigators in different countries, and independent of Carnot, laid hold of the same thought. The first who saw truly the general law here referred to, and expressed it correctly, was a German physician, J. R. Mayer of Heilbronn, in the year 1842. A little later, in 1843, a Dane named Colding presented a memoir to the Academy of Copenhagen, in which the same law found utterance, and some experiments were described for its further corroboration. In England, Joule began about the same time to make experiments having reference to the same subject. We often find, in the case of questions to the solution of which the development of science points, that several heads, quite independent of each other, generate exactly the same series of reflections.

I myself, without being acquainted with either Mayer or Colding, and having first made the acquaintance of Joule's experiments at the end of my investigation, followed the same path. I endeavoured to ascertain all the relations between the different natural processes, which followed from our regarding them from the above point of

Starting from each of these different manifestations of natural forces, we can set every other in motion, for the most part not in one way merely, but in many ways. It is here as with the weaver's web,—

> Where a step stirs a thousand threads,
> The shuttles shoot from side to side,
> The fibres flow unseen,
> And one shock strikes a thousand combinations.

Now it is clear that if by any means we could succeed, as the above American professed to have done, by mechanical forces, in exciting chemical, electrical, or other natural processes, which, by any circuit whatever, and without altering permanently the active masses in the machine, could produce mechanical force in greater quantity than that at first applied, a portion of the work thus gained might be made use of to keep the machine in motion, while the rest of the work might be applied to any other purpose whatever. The problem was to find, in the complicated net of reciprocal actions, a track through chemical, electrical, magnetical, and thermic processes, back to mechanical actions, which might be followed with a final gain of mechanical work: thus would the perpetual motion be found.

But, warned by the futility of former experiments, the public had become wiser. On the whole, people did not seek much after combinations which promised to furnish a perpetual motion, but the question was inverted. It was no more asked, How can I make use of the known and unknown relations of natural forces so as to construct a perpetual motion? but it was asked, If a perpetual motion be impossible, what are the relations which must subsist between natural forces? Everything was gained by this inversion of the question. The relations of natural forces rendered necessary by the above assumption, might

be easily and completely stated. It was found that all known relations of forces harmonise with the consequences of that assumption, and a series of unknown relations were discovered at the same time, the correctness of which remained to be proved. If a single one of them could be proved false, then a perpetual motion would be possible.

The first who endeavoured to travel this way was a Frenchman named Carnot, in the year 1824. In spite of a too limited conception of his subject, and an incorrect view as to the nature of heat, which led him to some erroneous conclusions, his experiment was not quite unsuccessful. He discovered a law which now bears his name, and to which I will return further on.

His labours remained for a long time without notice, and it was not till eighteen years afterwards, that is in 1842, that different investigators in different countries, and independent of Carnot, laid hold of the same thought. The first who saw truly the general law here referred to, and expressed it correctly, was a German physician, J. R. Mayer of Heilbronn, in the year 1842. A little later, in 1843, a Dane named Colding presented a memoir to the Academy of Copenhagen, in which the same law found utterance, and some experiments were described for its further corroboration. In England, Joule began about the same time to make experiments having reference to the same subject. We often find, in the case of questions to the solution of which the development of science points, that several heads, quite independent of each other, generate exactly the same series of reflections.

I myself, without being acquainted with either Mayer or Colding, and having first made the acquaintance of Joule's experiments at the end of my investigation, followed the same path. I endeavoured to ascertain all the relations between the different natural processes, which followed from our regarding them from the above point of

Starting from each of these different manifestations of natural forces, we can set every other in motion, for the most part not in one way merely, but in many ways. It is here as with the weaver's web,—

> Where a step stirs a thousand threads,
> The shuttles shoot from side to side,
> The fibres flow unseen,
> And one shock strikes a thousand combinations.

Now it is clear that if by any means we could succeed, as the above American professed to have done, by mechanical forces, in exciting chemical, electrical, or other natural processes, which, by any circuit whatever, and without altering permanently the active masses in the machine, could produce mechanical force in greater quantity than that at first applied, a portion of the work thus gained might be made use of to keep the machine in motion, while the rest of the work might be applied to any other purpose whatever. The problem was to find, in the complicated net of reciprocal actions, a track through chemical, electrical, magnetical, and thermic processes, back to mechanical actions, which might be followed with a final gain of mechanical work: thus would the perpetual motion be found.

But, warned by the futility of former experiments, the public had become wiser. On the whole, people did not seek much after combinations which promised to furnish a perpetual motion, but the question was inverted. It was no more asked, How can I make use of the known and unknown relations of natural forces so as to construct a perpetual motion? but it was asked, If a perpetual motion be impossible, what are the relations which must subsist between natural forces? Everything was gained by this inversion of the question. The relations of natural forces rendered necessary by the above assumption, might

be easily and completely stated. It was found that all known relations of forces harmonise with the consequences of that assumption, and a series of unknown relations were discovered at the same time, the correctness of which remained to be proved. If a single one of them could be proved false, then a perpetual motion would be possible.

The first who endeavoured to travel this way was a Frenchman named Carnot, in the year 1824. In spite of a too limited conception of his subject, and an incorrect view as to the nature of heat, which led him to some erroneous conclusions, his experiment was not quite unsuccessful. He discovered a law which now bears his name, and to which I will return further on.

His labours remained for a long time without notice, and it was not till eighteen years afterwards, that is in 1842, that different investigators in different countries, and independent of Carnot, laid hold of the same thought. The first who saw truly the general law here referred to, and expressed it correctly, was a German physician, J. R. Mayer of Heilbronn, in the year 1842. A little later, in 1843, a Dane named Colding presented a memoir to the Academy of Copenhagen, in which the same law found utterance, and some experiments were described for its further corroboration. In England, Joule began about the same time to make experiments having reference to the same subject. We often find, in the case of questions to the solution of which the development of science points, that several heads, quite independent of each other, generate exactly the same series of reflections.

I myself, without being acquainted with either Mayer or Colding, and having first made the acquaintance of Joule's experiments at the end of my investigation, followed the same path. I endeavoured to ascertain all the relations between the different natural processes, which followed from our regarding them from the above point of

Starting from each of these different manifestations of natural forces, we can set every other in motion, for the most part not in one way merely, but in many ways. It is here as with the weaver's web,—

> Where a step stirs a thousand threads,
> The shuttles shoot from side to side,
> The fibres flow unseen,
> And one shock strikes a thousand combinations.

Now it is clear that if by any means we could succeed, as the above American professed to have done, by mechanical forces, in exciting chemical, electrical, or other natural processes, which, by any circuit whatever, and without altering permanently the active masses in the machine, could produce mechanical force in greater quantity than that at first applied, a portion of the work thus gained might be made use of to keep the machine in motion, while the rest of the work might be applied to any other purpose whatever. The problem was to find, in the complicated net of reciprocal actions, a track through chemical, electrical, magnetical, and thermic processes, back to mechanical actions, which might be followed with a final gain of mechanical work: thus would the perpetual motion be found.

But, warned by the futility of former experiments, the public had become wiser. On the whole, people did not seek much after combinations which promised to furnish a perpetual motion, but the question was inverted. It was no more asked, How can I make use of the known and unknown relations of natural forces so as to construct a perpetual motion? but it was asked, If a perpetual motion be impossible, what are the relations which must subsist between natural forces? Everything was gained by this inversion of the question. The relations of natural forces rendered necessary by the above assumption, might

be easily and completely stated. It was found that all known relations of forces harmonise with the consequences of that assumption, and a series of unknown relations were discovered at the same time, the correctness of which remained to be proved. If a single one of them could be proved false, then a perpetual motion would be possible.

The first who endeavoured to travel this way was a Frenchman named Carnot, in the year 1824. In spite of a too limited conception of his subject, and an incorrect view as to the nature of heat, which led him to some erroneous conclusions, his experiment was not quite unsuccessful. He discovered a law which now bears his name, and to which I will return further on.

His labours remained for a long time without notice, and it was not till eighteen years afterwards, that is in 1842, that different investigators in different countries, and independent of Carnot, laid hold of the same thought. The first who saw truly the general law here referred to, and expressed it correctly, was a German physician, J. R. Mayer of Heilbronn, in the year 1842. A little later, in 1843, a Dane named Colding presented a memoir to the Academy of Copenhagen, in which the same law found utterance, and some experiments were described for its further corroboration. In England, Joule began about the same time to make experiments having reference to the same subject. We often find, in the case of questions to the solution of which the development of science points, that several heads, quite independent of each other, generate exactly the same series of reflections.

I myself, without being acquainted with either Mayer or Colding, and having first made the acquaintance of Joule's experiments at the end of my investigation, followed the same path. I endeavoured to ascertain all the relations between the different natural processes, which followed from our regarding them from the above point of

Starting from each of these different manifestations of natural forces, we can set every other in motion, for the most part not in one way merely, but in many ways. It is here as with the weaver's web,—

> Where a step stirs a thousand threads,
> The shuttles shoot from side to side,
> The fibres flow unseen,
> And one shock strikes a thousand combinations.

Now it is clear that if by any means we could succeed, as the above American professed to have done, by mechanical forces, in exciting chemical, electrical, or other natural processes, which, by any circuit whatever, and without altering permanently the active masses in the machine, could produce mechanical force in greater quantity than that at first applied, a portion of the work thus gained might be made use of to keep the machine in motion, while the rest of the work might be applied to any other purpose whatever. The problem was to find, in the complicated net of reciprocal actions, a track through chemical, electrical, magnetical, and thermic processes, back to mechanical actions, which might be followed with a final gain of mechanical work: thus would the perpetual motion be found.

But, warned by the futility of former experiments, the public had become wiser. On the whole, people did not seek much after combinations which promised to furnish a perpetual motion, but the question was inverted. It was no more asked, How can I make use of the known and unknown relations of natural forces so as to construct a perpetual motion? but it was asked, If a perpetual motion be impossible, what are the relations which must subsist between natural forces? Everything was gained by this inversion of the question. The relations of natural forces rendered necessary by the above assumption, might

be easily and completely stated. It was found that all known relations of forces harmonise with the consequences of that assumption, and a series of unknown relations were discovered at the same time, the correctness of which remained to be proved. If a single one of them could be proved false, then a perpetual motion would be possible.

The first who endeavoured to travel this way was a Frenchman named Carnot, in the year 1824. In spite of a too limited conception of his subject, and an incorrect view as to the nature of heat, which led him to some erroneous conclusions, his experiment was not quite unsuccessful. He discovered a law which now bears his name, and to which I will return further on.

His labours remained for a long time without notice, and it was not till eighteen years afterwards, that is in 1842, that different investigators in different countries, and independent of Carnot, laid hold of the same thought. The first who saw truly the general law here referred to, and expressed it correctly, was a German physician, J. R. Mayer of Heilbronn, in the year 1842. A little later, in 1843, a Dane named Colding presented a memoir to the Academy of Copenhagen, in which the same law found utterance, and some experiments were described for its further corroboration. In England, Joule began about the same time to make experiments having reference to the same subject. We often find, in the case of questions to the solution of which the development of science points, that several heads, quite independent of each other, generate exactly the same series of reflections.

I myself, without being acquainted with either Mayer or Colding, and having first made the acquaintance of Joule's experiments at the end of my investigation, followed the same path. I endeavoured to ascertain all the relations between the different natural processes, which followed from our regarding them from the above point of

Starting from each of these different manifestations of natural forces, we can set every other in motion, for the most part not in one way merely, but in many ways. It is here as with the weaver's web,—

> Where a step stirs a thousand threads,
> The shuttles shoot from side to side,
> The fibres flow unseen,
> And one shock strikes a thousand combinations.

Now it is clear that if by any means we could succeed, as the above American professed to have done, by mechanical forces, in exciting chemical, electrical, or other natural processes, which, by any circuit whatever, and without altering permanently the active masses in the machine, could produce mechanical force in greater quantity than that at first applied, a portion of the work thus gained might be made use of to keep the machine in motion, while the rest of the work might be applied to any other purpose whatever. The problem was to find, in the complicated net of reciprocal actions, a track through chemical, electrical, magnetical, and thermic processes, back to mechanical actions, which might be followed with a final gain of mechanical work: thus would the perpetual motion be found.

But, warned by the futility of former experiments, the public had become wiser. On the whole, people did not seek much after combinations which promised to furnish a perpetual motion, but the question was inverted. It was no more asked, How can I make use of the known and unknown relations of natural forces so as to construct a perpetual motion? but it was asked, If a perpetual motion be impossible, what are the relations which must subsist between natural forces? Everything was gained by this inversion of the question. The relations of natural forces rendered necessary by the above assumption, might

be easily and completely stated. It was found that all known relations of forces harmonise with the consequences of that assumption, and a series of unknown relations were discovered at the same time, the correctness of which remained to be proved. If a single one of them could be proved false, then a perpetual motion would be possible.

The first who endeavoured to travel this way was a Frenchman named Carnot, in the year 1824. In spite of a too limited conception of his subject, and an incorrect view as to the nature of heat, which led him to some erroneous conclusions, his experiment was not quite unsuccessful. He discovered a law which now bears his name, and to which I will return further on.

His labours remained for a long time without notice, and it was not till eighteen years afterwards, that is in 1842, that different investigators in different countries, and independent of Carnot, laid hold of the same thought. The first who saw truly the general law here referred to, and expressed it correctly, was a German physician, J. R. Mayer of Heilbronn, in the year 1842. A little later, in 1843, a Dane named Colding presented a memoir to the Academy of Copenhagen, in which the same law found utterance, and some experiments were described for its further corroboration. In England, Joule began about the same time to make experiments having reference to the same subject. We often find, in the case of questions to the solution of which the development of science points, that several heads, quite independent of each other, generate exactly the same series of reflections.

I myself, without being acquainted with either Mayer or Colding, and having first made the acquaintance of Joule's experiments at the end of my investigation, followed the same path. I endeavoured to ascertain all the relations between the different natural processes, which followed from our regarding them from the above point of

view. My inquiry was made public in 1847, in a small pamphlet bearing the title, 'On the Conservation of Force.'[1]

Since that time the interest of the scientific public for this subject has gradually augmented, particularly in England, of which I had an opportunity of convincing myself during a visit last summer. A great number of the essential consequences of the above manner of viewing the subject, the proof of which was wanting when the first theoretic notions were published, have since been confirmed by experiment, particularly by those of Joule; and during the last year the most eminent physicist of France, Regnault, has adopted the new mode of regarding the question, and by fresh investigations on the specific heat of gases has contributed much to its support. For some important consequences the experimental proof is still wanting, but the number of confirmations is so predominant, that I have not deemed it premature to bring the subject before even a non-scientific audience.

How the question has been decided you may already infer from what has been stated. In the series of natural processes there is no circuit to be found, by which mechanical force can be gained without a corresponding consumption. The perpetual motion remains impossible. Our reflections, however, gain thereby a higher interest.

We have thus far regarded the development of force by natural processes, only in its relation to its usefulness to man, as mechanical force. You now see that we have arrived at a general law, which holds good wholly independent of the application which man makes of natural forces; we must therefore make the expression of our law correspond to this more general significance. It is in the first place clear, that the work which, by any natural pro-

[1] There is a translation of this important Essay in the *Scientific Memoirs*, New Series, p. 114.—J. T.

cess whatever, is performed under favourable conditions by a machine, and which may be measured in the way already indicated, may be used as a measure of force common to all. Further, the important question arises, If the quantity of force cannot be augmented except by corresponding consumption, can it be diminished or lost? For the purposes of our machines it certainly can, if we neglect the opportunity to convert natural processes to use, but as investigation has proved, not for nature as a whole.

In the collision and friction of bodies against each other, the mechanics of former years assumed simply that living force was lost. But I have already stated that each collision and each act of friction generates heat; and, moreover, Joule has established by experiment the important law, that for every foot-pound of force which is lost a definite quantity of heat is always generated, and that when work is performed by the consumption of heat, for each foot-pound thus gained a definite quantity of heat disappears. The quantity of heat necessary to raise the temperature of a pound of water a degree of the Centigrade thermometer, corresponds to a mechanical force by which a pound weight would be raised to the height of 1,350 feet: we name this quantity the mechanical equivalent of heat. I may mention here that these facts conduct of necessity to the conclusion, that heat is not, as was formerly imagined, a fine imponderable substance, but that, like light, it is a peculiar shivering motion of the ultimate particles of bodies. In collision and friction, according to this manner of viewing the subject, the motion of the mass of a body which is apparently lost is converted into a motion of the ultimate particles of the body; and conversely, when mechanical force is generated by heat, the motion of the ultimate particles is converted into a motion of the mass.

Chemical combinations generate heat, and the quantity

of this heat is totally independent of the time and steps through which the combination has been effected, provided that other actions are not at the same time brought into play. If, however, mechanical work is at the same time accomplished, as in the case of the steam-engine, we obtain as much less heat as is equivalent to this work. The quantity of work produced by chemical force is in general very great. A pound of the purest coal gives, when burnt, sufficient heat to raise the temperature of 8,086 pounds of water one degree of the Centigrade thermometer; from this we can calculate that the magnitude of the chemical force of attraction between the particles of a pound of coal and the quantity of oxygen that corresponds to it, is capable of lifting a weight of 100 pounds to a height of twenty miles. Unfortunately, in our steam-engines we have hitherto been able to gain only the smallest portion of this work, the greater part is lost in the shape of heat. The best expansive engines give back as mechanical work only 18 per cent. of the heat generated by the fuel.

From a similar investigation of all the other known physical and chemical processes, we arrive at the conclusion that Nature as a whole possesses a store of force which cannot in any way be either increased or diminished, and that therefore the quantity of force in Nature is just as eternal and unalterable as the quantity of matter. Expressed in this form, I have named the general law 'The Principle of the Conservation of Force.'

We cannot create mechanical force, but we may help ourselves from the general storehouse of Nature. The brook and the wind, which drive our mills, the forest and the coal-bed, which supply our steam-engines and warm our rooms, are to us the bearers of a small portion of the great natural supply which we draw upon for our purposes, and the actions of which we can apply as we think

fit. The possessor of a mill claims the gravity of the descending rivulet, or the living force of the moving wind, as his possession. These portions of the store of Nature are what give his property its chief value.

Further, from the fact that no portion of force can be absolutely lost, it does not follow that a portion may not be inapplicable to human purposes. In this respect the inferences drawn by William Thomson from the law of Carnot are of importance. This law, which was discovered by Carnot during his endeavours to ascertain the relations between heat and mechanical force, which, however, by no means belongs to the necessary consequences of the conservation of force, and which Clausius was the first to modify in such a manner that it no longer contradicted the above general law, expresses a certain relation between the compressibility, the capacity for heat, and the expansion by heat of all bodies. It is not yet completely proved in all directions, but some remarkable deductions having been drawn from it, and afterwards proved to be facts by experiment, it has attained thereby the highest degree of probability. Besides the mathematical form in which the law was first expressed by Carnot, we can give it the following more general expression:—'Only when heat passes from a warmer to a colder body, and even then only partially, can it be converted into mechanical work.'

The heat of a body which we cannot cool further, cannot be changed into another form of force—into electric or chemical force for example. Thus in our steam-engines we convert a portion of the heat of the glowing coal into work, by permitting it to pass to the less warm water of the boiler. If, however, all the bodies in Nature had the same temperature, it would be impossible to convert any portion of their heat into mechanical work. According to this we can divide the total force store of the universe into two parts, one of which is heat,

and must continue to be such; the other, to which a portion of the heat of the warmer bodies, and the total supply of chemical, mechanical, electrical, and magnetical forces belong, is capable of the most varied changes of form, and constitutes the whole wealth of change which takes place in Nature.

But the heat of the warmer bodies strives perpetually to pass to bodies less warm by radiation and conduction, and thus to establish an equilibrium of temperature. At each motion of a terrestrial body a portion of mechanical force passes by friction or collision into heat, of which only a part can be converted back again into mechanical force. This is also generally the case in every electrical and chemical process. From this it follows that the first portion of the store of force, the unchangeable heat, is augmented by every natural process, while the second portion, mechanical, electrical, and chemical force, must be diminished; so that if the universe be delivered over to the undisturbed action of its physical processes, all force will finally pass into the form of heat, and all heat come into a state of equilibrium. Then all possibility of a further change would be at an end, and the complete cessation of all natural processes must set in. The life of men, animals, and plants could not of course continue if the sun had lost his high temperature, and with it his light,—if all the components of the earth's surface had closed those combinations which their affinities demand. In short, the universe from that time forward would be condemned to a state of eternal rest.

These consequences of the law of Carnot are, of course, only valid provided that the law, when sufficiently tested, proves to be universally correct. In the mean time there is little prospect of the law being proved incorrect. At all events, we must admire the sagacity of Thomson, who, in the letters of a long-known little mathematical for-

mula, which only speaks of the heat, volume, and pressure of bodies, was able to discern consequences which threatened the universe, though certainly after an infinite period of time, with eternal death.

I have already given you notice that our path lay through a thorny and unrefreshing field of mathematico-mechanical developments. We have now left this portion of our road behind us. The general principle which I have sought to lay before you has conducted us to a point from which our view is a wide one; and aided by this principle, we can now at pleasure regard this or the other side of the surrounding world according as our interest in the matter leads us. A glance into the narrow laboratory of the physicist, with its small appliances and complicated abstractions, will not be so attractive as a glance at the wide heaven above us, the clouds, the rivers, the woods, and the living beings around us. While regarding the laws which have been deduced from the physical processes of terrestrial bodies as applicable also to the heavenly bodies, let me remind you that the same force which, acting at the earth's surface, we call gravity (*Schwere*), acts as gravitation in the celestial spaces, and also manifests its power in the motion of the immeasurably distant double stars, which are governed by exactly the same laws as those subsisting between the earth and moon; that therefore the light and heat of terrestrial bodies do not in any way differ essentially from those of the sun or of the most distant fixed star; that the meteoric stones which sometimes fall from external space upon the earth are composed of exactly the same simple chemical substances as those with which we are acquainted. We need, therefore, feel no scruple in granting that general laws to which all terrestrial natural processes are subject are also valid for other bodies than the earth. We will, therefore, make use of our law to glance over the house-

hold of the universe with respect to the store of force, capable of action, which it possesses.

A number of singular peculiarities in the structure of our planetary system indicate that it was once a connected mass, with a uniform motion of rotation. Without such an assumption it is impossible to explain why all the planets move in the same direction round the sun, why they all rotate in the same direction round their axes, why the planes of their orbits and those of their satellites and rings all nearly coincide, why all their orbits differ but little from circles, and much besides. From these remaining indications of a former state astronomers have shaped an hypothesis regarding the formation of our planetary system, which, although from the nature of the case it must ever remain an hypothesis, still in its special traits is so well supported by analogy, that it certainly deserves our attention; and the more so, as this notion in our own home, and within the walls of this town,[1] first found utterance. It was Kant who, feeling great interest in the physical description of the earth and the planetary system, undertook the labour of studying the works of Newton; and, as an evidence of the depth to which he had penetrated into the fundamental ideas of Newton, seized the notion that the same attractive force of all ponderable matter which now supports the motion of the planets must also aforetime have been able to form from matter loosely scattered in space the planetary system. Afterwards, and independent of Kant, Laplace, the great author of the 'Mécanique céleste,' laid hold of the same thought, and introduced it among astronomers.

The commencement of our planetary system, including the sun, must, according to this, be regarded as an immense nebulous mass which filled the portion of space now occupied by our system far beyond the

[1] Königsberg.

limits of Neptune, our most distant planet. Even now we discern in distant regions of the firmament nebulous patches the light of which, as spectrum analysis teaches, is the light of ignited gases; and in their spectra we see more especially those bright lines which are produced by ignited hydrogen and by ignited nitrogen. Within our system, also, comets, the crowds of shooting stars, and the zodiacal light exhibit distinct traces of matter dispersed like powder, which moves, however, according to the law of gravitation, and is, at all events, partially retarded by the larger bodies and incorporated in them. The latter undoubtedly happens with the shooting stars and meteoric stones which come within the range of our atmosphere.

If we calculate the density of the mass of our planetary system, according to the above assumption, for the time when it was a nebulous sphere, which reached to the path of the outermost planet, we should find that it would require several millions of cubic miles of such matter to weigh a single grain.

The general attractive force of all matter must, however, impel these masses to approach each other, and to condense, so that the nebulous sphere became incessantly smaller, by which, according to mechanical laws, a motion of rotation originally slow, and the existence of which must be assumed, would gradually become quicker and quicker. By the centrifugal force, which must act most energetically in the neighbourhood of the equator of the nebulous sphere, masses could from time to time be torn away, which afterwards would continue their courses separate from the main mass, forming themselves into single planets, or, similar to the great original sphere, into planets with satellites and rings, until finally the principal mass condensed itself into the sun. With regard to the origin of heat and light this theory originally gave no information.

When the nebulous chaos first separated itself from other fixed star masses it must not only have contained all kinds of matter which was to constitute the future planetary system, but also, in accordance with our new law, the whole store of force which at a future time ought to unfold therein its wealth of actions. Indeed, in this respect an immense dower was bestowed in the shape of the general attraction of all the particles for each other. This force, which on the earth exerts itself as gravity, acts in the heavenly spaces as gravitation. As terrestrial gravity when it draws a weight downwards performs work and generates *vis viva*, so also the heavenly bodies do the same when they draw two portions of matter from distant regions of space towards each other.

The chemical forces must have been also present, ready to act; but as these forces can only come into operation by the most intimate contact of the different masses, condensation must have taken place before the play of chemical forces began.

Whether a still further supply of force in the shape of heat was present at the commencement we do not know. At all events, by aid of the law of the equivalence of heat and work, we find in the mechanical forces existing at the time to which we refer such a rich source of heat and light, that there is no necessity whatever to take refuge in the idea of a store of these forces originally existing. When, through condensation of the masses, their particles came into collision and clung to each other, the *vis viva* of their motion would be thereby annihilated, and must reappear as heat. Already in old theories it has been calculated that cosmical masses must generate heat by their collision, but it was far from anybody's thought to make even a guess at the amount of heat to be generated in this way. At present we can give definite numerical values with certainty.

Let us make this addition to our assumption—that, at the commencement, the density of the nebulous matter was a vanishing quantity as compared with the present density of the sun and planets: we can then calculate how much work has been performed by the condensation; we can further calculate how much of this work still exists in the form of mechanical force, as attraction of the planets towards the sun, and as *vis viva* of their motion, and find by this how much of the force has been converted into heat.

The result of this calculation[1] is, that only about the 454th part of the original mechanical force remains as such, and that the remainder, converted into heat, would be sufficient to raise a mass of water equal to the sun and planets taken together, not less than twenty-eight millions of degrees of the Centigrade scale. For the sake of comparison, I will mention that the highest temperature which we can produce by the oxyhydrogen blowpipe, which is sufficient to fuse and vaporise even platinum, and which but few bodies can endure without melting, is estimated at about 2,000 degrees. Of the action of a temperature of twenty-eight millions of such degrees we can form no notion. If the mass of our entire system were pure coal, by the combustion of the whole of it only the 3,500th part of the above quantity would be generated. This is also clear, that such a great development of heat must have presented the greatest obstacle to the speedy union of the masses; that the greater part of the heat must have been diffused by radiation into space, before the masses could form bodies possessing the present density of the sun and planets, and that these bodies must once have been in a state of fiery fluidity. This notion is corroborated by the geological phænomena of our planet; and with regard to the other planetary bodies, the flat-

[1] See note on page 193.

tened form of the sphere, which is the form of equilibrium of a fluid mass, is indicative of a former state of fluidity. If I thus permit an immense quantity of heat to disappear without compensation from our system, the principle of the conservation of force is not thereby invaded. Certainly for our planet it is lost, but not for the universe. It has proceeded outwards, and daily proceeds outwards into infinite space; and we know not whether the medium which transmits the undulations of light and heat possesses an end where the rays must return, or whether they eternally pursue their way through infinitude.

The store of force at present possessed by our system is also equivalent to immense quantities of heat. If our earth were by a sudden shock brought to rest in her orbit —which is not to be feared in the existing arrangement of our system—by such a shock a quantity of heat would be generated equal to that produced by the combustion of fourteen such earths of solid coal. Making the most unfavourable assumption as to its capacity for heat—that is, placing it equal to that of water—the mass of the earth would thereby be heated 11,200 degrees; it would, therefore, be quite fused, and for the most part converted into vapour. If, then, the earth, after having been thus brought to rest, should fall into the sun—which, of course, would be the case—the quantity of heat developed by the shock would be 400 times greater.

Even now from time to time such a process is repeated on a small scale. There can hardly be a doubt that meteors, fireballs, and meteoric stones are masses which belong to the universe, and before coming into the domain of our earth, moved like the planets round the sun. Only when they enter our atmosphere do they become visible and fall sometimes to the earth. In order to explain the emission of light by these bodies, and the fact that for some time after their descent they are very

hot, the friction was long ago thought of which they experience in passing through the air. We can now calculate that a velocity of 3,000 feet a second, supposing the whole of the friction to be expended in heating the solid mass, would raise a piece of meteoric iron 1,000° C. in temperature, or, in other words, to a vivid red heat. Now the average velocity of the meteors seems to be thirty to fifty times the above amount. To compensate this, however, the greater portion of the heat is doubtless carried away by the condensed mass of air which the meteor drives before it. It is known that bright meteors generally leave a luminous trail behind them, which probably consists of severed portions of the red-hot surfaces. Meteoric masses which fall to the earth often burst with a violent explosion, which may be regarded as a result of the quick heating. The newly-fallen pieces have been for the most part found hot, but not red-hot, which is easily explainable by the circumstance, that during the short time occupied by the meteor in passing through the atmosphere, only a thin superficial layer is heated to redness, while but a small quantity of heat has been able to penetrate to the interior of the mass. For this reason the red heat can speedily disappear.

Thus has the falling of the meteoric stone, the minute remnant of processes which seem to have played an important part in the formation of the heavenly bodies, conducted us to the present time, where we pass from the darkness of hypothetical views to the brightness of knowledge. In what we have said, however, all that is hypothetical is the assumption of Kant and Laplace, that the masses of our system were once distributed as nebulæ in space.

On account of the rarity of the case, we will still further remark in what close coincidence the results of science here stand with the earlier legends of the human

family, and the forebodings of poetic fancy. The cosmogony of ancient nations generally commences with chaos and darkness. Thus for example Mephistopheles says:—

> Part of the Part am I, once All, in primal night,
> Part of the Darkness which brought forth the Light,
> The haughty Light, which now disputes the space,
> And claims of Mother Night her ancient place.

Neither is the Mosaic tradition very divergent, particularly when we remember that that which Moses names heaven, is different from the blue dome above us, and is synonymous with space, and that the unformed earth and the waters of the great deep, which were afterwards divided into waters above the firmament and waters below the firmament, resembled the chaotic components of the world:—

'In the beginning God created the heaven and the earth.

'And the earth was without form, and void; and darkness was upon the face of the deep. And the spirit of God moved upon the face of the waters.'

And just as in nebulous sphere, just become luminous, and in the new red-hot liquid earth of our modern cosmogony light was not yet divided into sun and stars, nor time into day and night, as it was after the earth had cooled.

'And God divided the light from the darkness.

'And God called the light day, and the darkness He called night. And the evening and the morning were the first day.'

And now, first, after the waters had been gathered together into the sea, and the earth had been laid dry, could plants and animals be formed.

Our earth bears still the unmistakeable traces of its old fiery fluid condition. The granite formations of her mountains exhibit a structure, which can only be pro-

duced by the crystallisation of fused masses. Investigation still shows that the temperature in mines and borings increases as we descend; and if this increase is uniform, at the depth of fifty miles a heat exists sufficient to fuse all our minerals. Even now our volcanoes project from time to time mighty masses of fused rocks from their interior, as a testimony of the heat which exists there. But the cooled crust of the earth has already become so thick, that, as may be shown by calculations of its conductive power, the heat coming to the surface from within, in comparison with that reaching the earth from the sun, is exceedingly small, and increases the temperature of the surface only about $\frac{1}{30}$th of a degree Centigrade; so that the remnant of the old store of force which is enclosed as heat within the bowels of the earth has a sensible influence upon the processes at the earth's surface only through the instrumentality of volcanic phænomena. Those processes owe their power almost wholly to the action of other heavenly bodies, particularly to the light and heat of the sun, and partly also, in the case of the tides, to the attraction of the sun and moon.

Most varied and numerous are the changes which we owe to the light and heat of the sun. The sun heats our atmosphere irregularly, the warm rarefied air ascends, while fresh cool air flows from the sides to supply its place: in this way winds are generated. This action is most powerful at the equator, the warm air of which incessantly flows in the upper regions of the atmosphere towards the poles; while just as persistently at the earth's surface, the trade-wind carries new and cool air to the equator. Without the heat of the sun, all winds must of necessity cease. Similar currents are produced by the same cause in the waters of the sea. Their power may be inferred from the influence which in some cases they exert upon climate. By them the warm

water of the Antilles is carried to the British Isles, and confers upon them a mild uniform warmth, and rich moisture; while, through similar causes, the floating ice of the North Pole is carried to the coast of Newfoundland and produces raw cold. Further, by the heat of the sun a portion of the water is converted into vapour, which rises in the atmosphere, is condensed to clouds, or falls in rain and snow upon the earth, collects in the form of springs, brooks, and rivers, and finally reaches the sea again, after having gnawed the rocks, carried away light earth, and thus performed its part in the geologic changes of the earth; perhaps besides all this it has driven our water-mill upon its way. If the heat of the sun were withdrawn, there would remain only a single motion of water, namely, the tides, which are produced by the attraction of the sun and moon.

How is it, now, with the motions and the work of organic beings? To the builders of the automata of the last century, men and animals appeared as clockwork which was never wound up, and created the force which they exerted out of nothing. They did not know how to establish a connexion between the nutriment consumed and the work generated. Since, however, we have learned to discern in the steam-engine this origin of mechanical force, we must inquire whether something similar does not hold good with regard to men. Indeed, the continuation of life is dependent on the consumption of nutritive materials: these are combustible substances, which, after digestion and being passed into the blood, actually undergo a slow combustion, and finally enter into almost the same combinations with the oxygen of the atmosphere that are produced in an open fire. As the quantity of heat generated by combustion is independent of the duration of the combustion and the steps in which it occurs, we can calculate from the mass of the

consumed material how much heat, or its equivalent work, is thereby generated in an animal body. Unfortunately, the difficulty of the experiments is still very great; but within those limits of accuracy which have been as yet attainable, the experiments show that the heat generated in the animal body corresponds to the amount which would be generated by the chemical processes. The animal body therefore does not differ from the steam-engine as regards the manner in which it obtains heat and force, but does differ from it in the manner in which the force gained is to be made use of. The body is, besides, more limited than the machine in the choice of its fuel; the latter could be heated with sugar, with starch-flour, and butter, just as well as with coal or wood; the animal body must dissolve its materials artificially, and distribute them through its system; it must, further, perpetually renew the used-up materials of its organs, and as it cannot itself create the matter necessary for this, the matter must come from without. Liebig was the first to point out these various uses of the consumed nutriment. As material for the perpetual renewal of the body, it seems that certain definite albuminous substances which appear in plants, and form the chief mass of the animal body, can alone be used. They form only a portion of the mass of nutriment taken daily; the remainder, sugar, starch, fat, are really only materials for warming, and are perhaps not to be superseded by coal, simply because the latter does not permit itself to be dissolved.

If, then, the processes in the animal body are not in this respect to be distinguished from inorganic processes, the question arises, whence comes the nutriment which constitutes the source of the body's force? The answer is, from the vegetable kingdom; for only the material of plants, or the flesh of herbivorous animals, can be

made use of for food. The animals which live on plants occupy a mean position between carnivorous animals, in which we reckon man, and vegetables, which the former could not make use of immediately as nutriment. In hay and grass the same nutritive substances are present as in meal and flour, but in less quantity. As, however, the digestive organs of man are not in a condition to extract the small quantity of the useful from the great excess of the insoluble, we submit, in the first place, these substances to the powerful digestion of the ox, permit the nourishment to store itself in the animal's body, in order in the end to gain it for ourselves in a more agreeable and useful form. In answer to our question, therefore, we are referred to the vegetable world. Now when what plants take in and what they give out are made the subjects of investigation, we find that the principal part of the former consists in the products of combustion which are generated by the animal. They take the consumed carbon given off in respiration, as carbonic acid, from the air, the consumed hydrogen as water, the nitrogen in its simplest and closest combination as ammonia; and from these materials, with the assistance of small ingredients which they take from the soil, they generate anew the compound combustible substances, albumen, sugar, oil, on which the animal subsists. Here, therefore, is a circuit which appears to be a perpetual store of force. Plants prepare fuel and nutriment, animals consume these, burn them slowly in their lungs, and from the products of combustion the plants again derive their nutriment. The latter is an eternal source of chemical, the former of mechanical forces. Would not the combination of both organic kingdoms produce the perpetual motion? We must not conclude hastily: further inquiry shows, that plants are capable of producing combustible substances only when they are

under the influence of the sun. A portion of the sun's rays exhibits a remarkable relation to chemical forces,—it can produce and destroy chemical combinations; and these rays, which for the most part are blue or violet, are called therefore chemical rays. We make use of their action in the production of photographs. Here compounds of silver are decomposed at the place where the sun's rays strike them. The same rays overpower in the green leaves of plants the strong chemical affinity of the carbon of the carbonic acid for oxygen, give back the latter free to the atmosphere, and accumulate the other, in combination with other bodies, as woody fibre, starch, oil, or resin. These chemically active rays of the sun disappear completely as soon as they encounter the green portions of the plants, and hence it is that in Daguerreotype images the green leaves of plants appear uniformly black. Inasmuch as the light coming from them does not contain the chemical rays, it is unable to act upon the silver compounds. But besides the blue and violet, the yellow rays play an important part in the growth of plants. They also are comparatively strongly absorbed by the leaves.

Hence a certain portion of force disappears from the sunlight, while combustible substances are generated and accumulated in plants; and we can assume it as very probable, that the former is the cause of the latter. I must indeed remark, that we are in possession of no experiments from which we might determine whether the *vis viva* of the sun's rays which have disappeared corresponds to the chemical forces accumulated during the same time; and as long as these experiments are wanting, we cannot regard the stated relation as a certainty. If this view should prove correct, we derive from it the flattering result, that all force, by means of which our bodies live and move, finds its source in the purest sun-

light; and hence we are all, in point of nobility, not behind the race of the great monarch of China, who heretofore alone called himself Son of the Sun. But it must also be conceded, that our lower fellow-beings, the frog and leech, share the same æthereal origin, as also the whole vegetable world, and even the fuel which comes to us from the ages past, as well as the youngest offspring of the forest with which we heat our stoves and set our machines in motion.

You see, then, that the immense wealth of ever-changing meteorological, climatic, geological, and organic processes of our earth are almost wholly preserved in action by the light- and heat-giving rays of the sun; and you see in this a remarkable example, how Proteus-like the effects of a single cause, under altered external conditions, may exhibit itself in nature. Besides these, the earth experiences an action of another kind from its central luminary, as well as from its satellite the moon, which exhibits itself in the remarkable phænomenon of the ebb and flow of the tide.

Each of these bodies excites, by its attraction upon the waters of the sea, two gigantic waves, which flow in the same direction round the world, as the attracting bodies themselves apparently do. The two waves of the moon, on account of her greater nearness, are about $3\frac{1}{2}$ times as large as those excited by the sun. One of these waves has its crest on the quarter of the earth's surface which is turned towards the moon, the other is at the opposite side. Both these quarters possess the flow of the tide, while the regions which lie between have the ebb. Although in the open sea the height of the tide amounts to only about three feet, and only in certain narrow channels, where the moving water is squeezed together, rises to thirty feet, the might of the phænomenon is nevertheless manifest from the calculation of Bessel, according to

which a quarter of the earth covered by the sea possesses, during the flow of the tide, about 22,000 cubic miles of water more than during the ebb, and that therefore such a mass of water must, in 6¼ hours, flow from one quarter of the earth to the other.

The phænomenon of the ebb and flow, as already recognised by Mayer, combined with the law of the conservation of force, stands in remarkable connexion with the question of the stability of our planetary system. The mechanical theory of the planetary motions discovered by Newton teaches, that if a solid body in absolute *vacuo*, attracted by the sun, move around him in the same manner as the planets, this motion will endure unchanged through all eternity.

Now we have actually not only one, but several such planets, which move around the sun, and by their mutual attraction create little changes and disturbances in each other's paths. Nevertheless Laplace, in his great work, the 'Mécanique céleste,' has proved that in our planetary system all these disturbances increase and diminish periodically, and can never exceed certain limits, so that by this cause the eternal existence of the planetary system is unendangered.

But I have already named two assumptions which must be made: first, that the celestial spaces must be absolutely empty; and secondly, that the sun and planets must be solid bodies. The first is at least the case as far as astronomical observations reach, for they have never been able to detect any retardation of the planets, such as would occur if they moved in a resisting medium. But on a body of less mass, the comet of Encke, changes are observed of such a nature: this comet describes ellipses round the sun which are becoming gradually smaller. If this kind of motion, which certainly corresponds to that through a resisting medium, be actually

due to the existence of such a medium, a time will come when the comet will strike the sun; and a similar end threatens all the planets, although after a time, the length of which baffles our imagination to conceive of it. But even should the existence of a resisting medium appear doubtful to us, there is no doubt that the planets are not wholly composed of solid materials which are inseparably bound together. Signs of the existence of an atmosphere are observed on the Sun, on Venus, Mars, Jupiter, and Saturn. Signs of water and ice upon Mars; and our earth has undoubtedly a fluid portion on its surface, and perhaps a still greater portion of fluid within it. The motions of the tides, however, produce friction, all friction destroys *vis viva*, and the loss in this case can only affect the *vis viva* of the planetary system. We come thereby to the unavoidable conclusion, that every tide, although with infinite slowness, still with certainty diminishes the store of mechanical force of the system; and as a consequence of this, the rotation of the planets in question round their axes must become more slow. The recent careful investigations of the moon's motion made by Hansen, Adams, and Delaunay, have proved that the earth does experience such a retardation. According to the former, the length of each sidereal day has increased since the time of Hipparchus by the $\frac{1}{81}$ part of a second, and the duration of a century by half a quarter of an hour; according to Adams and Sir W. Thomson, the increase has been almost twice as great. A clock which went right at the beginning of a century, would be twenty-two seconds in advance of the earth at the end of the century. Laplace had denied the existence of such a retardation in the case of the earth; to ascertain the amount, the theory of lunar motion required a greater development than was possible in his time. The final consequence would be, but after millions of years, if in

the mean time the ocean did not become frozen, that one side of the earth would be constantly turned towards the sun, and enjoy a perpetual day, whereas the opposite side would be involved in eternal night. Such a position we observe in our moon with regard to the earth, and also in the case of the satellites as regards their planets; it is, perhaps, due to the action of the mighty ebb and flow to which these bodies, in the time of their fiery fluid condition, were subjected.

I would not have brought forward these conclusions, which again plunge us in the most distant future, if they were not unavoidable. Physico-mechanical laws are, as it were, the telescopes of our spiritual eye, which can penetrate into the deepest night of time, past and to come.

Another essential question as regards the future of our planetary system has reference to its future temperature and illumination. As the internal heat of the earth has but little influence on the temperature of the surface, the heat of the sun is the only thing which essentially affects the question. The quantity of heat falling from the sun during a given time upon a given portion of the earth's surface may be measured, and from this it can be calculated how much heat in a given time is sent out from the entire sun. Such measurements have been made by the French physicist Pouillet, and it has been found that the sun gives out a quantity of heat per hour equal to that which a layer of the densest coal 10 feet thick would give out by its combustion; and hence in a year a quantity equal to the combustion of a layer of 17 miles. If this heat were drawn uniformly from the entire mass of the sun, its temperature would only be diminished thereby $1\frac{1}{3}$ of a degree Centigrade per year, assuming its capacity for heat to be equal to that of water. These results can give us an idea of the magnitude of the

emission, in relation to the surface and mass of the sun; but they cannot inform us whether the sun radiates heat as a glowing body, which since its formation has its heat accumulated within it, or whether a new generation of heat by chemical processes is continually taking place at the sun's surface. At all events, the law of the conservation of force teaches us that no process analogous to those known at the surface of the earth can supply for eternity an inexhaustible amount of light and heat to the sun. But the same law also teaches that the store of force at present existing, as heat, or as what may become heat, is sufficient for an immeasurable time. With regard to the store of chemical force in the sun, we can form no conjecture, and the store of heat there existing can only be determined by very uncertain estimations. If, however, we adopt the very probable view, that the remarkably small density of so large a body is caused by its high temperature, and may become greater in time, it may be calculated that if the diameter of the sun were diminished only the ten-thousandth part of its present length, by this act a sufficient quantity of heat would be generated to cover the total emission for 2,100 years. So small a change it would be difficult to detect even by the finest astronomical observations.

Indeed, from the commencement of the period during which we possess historic accounts, that is, for a period of about 4,000 years, the temperature of the earth has not sensibly diminished. From these old ages we have certainly no thermometric observations, but we have information regarding the distribution of certain cultivated plants, the vine, the olive tree, which are very sensitive to changes of the mean annual temperature, and we find that these plants at the present moment have the same limits of distribution that they had in the times of Abraham and Homer; from which we may infer backwards the constancy of the climate.

In opposition to this it has been urged, that here in Prussia the German knights in former times cultivated the vine, cellared their own wine and drank it, which is no longer possible. From this the conclusion has been drawn, that the heat of our climate has diminished since the time referred to. Against this, however, Dove has cited the reports of ancient chroniclers, according to which, in some peculiarly hot years, the Prussian grape possessed somewhat less than its usual quantity of acid. The fact also speaks not so much for the climate of the country as for the throats of the German drinkers.

But even though the force store of our planetary system is so immensely great, that by the incessant emission which has occurred during the period of human history it has not been sensibly diminished, even though the length of the time which must flow by before a sensible change in the state of our planetary system occurs is totally incapable of measurement, still the inexorable laws of mechanics indicate that this store of force, which can only suffer loss and not gain, must be finally exhausted. Shall we terrify ourselves by this thought? Men are in the habit of measuring the greatness and the wisdom of the universe by the duration and the profit which it promises to their own race; but the past history of the earth already shows what an insignificant moment the duration of the existence of our race upon it constitutes. A Nineveh vessel, a Roman sword, awake in us the conception of grey antiquity. What the museums of Europe show us of the remains of Egypt and Assyria we gaze upon with silent astonishment, and despair of being able to carry our thoughts back to a period so remote. Still must the human race have existed for ages, and multiplied itself before the Pyramids or Nineveh could have been erected. We estimate the duration of human history at 6,000 years; but immeasurable as this time may

appear to us, what is it in comparison with the time during which the earth carried successive series of rank plants and mighty animals, and no men; during which in our neighbourhood the amber-tree bloomed, and dropped its costly gum on the earth and in the sea; when in Siberia, Europe, and North America groves of tropical palms flourished; where gigantic lizards, and after them elephants, whose mighty remains we still find buried in the earth, found a home? Different geologists, proceeding from different premises, have sought to estimate the duration of the above-named creative period, and vary from a million to nine million years. The time during which the earth generated organic beings is again small when compared with the ages during which the world was a ball of fused rocks. For the duration of its cooling from 2,000° to 200° Centigrade the experiments of Bishop upon basalt show that about 350 millions of years would be necessary. And with regard to the time during which the first nebulous mass condensed into our planetary system, our most daring conjectures must cease. The history of man, therefore, is but a short ripple in the ocean of time. For a much longer series of years than that during which he has already occupied this world, the existence of the present state of inorganic nature favourable to the duration of man seems to be secured, so that for ourselves and for long generations after us we have nothing to fear. But the same forces of air and water, and of the volcanic interior, which produced former geological revolutions, and buried one series of living forms after another, act still upon the earth's crust. They more probably will bring about the last day of the human race than those distant cosmical alterations of which we have spoken, forcing us perhaps to make way for new and more complete living forms, as the lizards and the mammoth

have given place to us and our fellow-creatures which now exist.

Thus the thread which was spun in darkness by those who sought a perpetual motion has conducted us to a universal law of nature, which radiates light into the distant nights of the beginning and of the end of the history of the universe. To our own race it permits a long but not an endless existence; it threatens it with a day of judgment, the dawn of which is still happily obscured. As each of us singly must endure the thought of his death, the race must endure the same. But above the forms of life gone by, the human race has higher moral problems before it, the bearer of which it is, and in the completion of which it fulfils its destiny.

NOTE TO PAGE 177.

I must here explain the calculation of the heat which must be produced by the assumed condensation of the bodies of our system from scattered nebulous matter. The other calculations, the results of which I have mentioned, are to be found partly in J. R. Mayer's papers, partly in Joule's communications, and partly by aid of the known facts and method of science: they are easily performed.

The measure of the work performed by the condensation of the mass from a state of infinitely small density is the potential of the condensed mass upon itself. For a sphere of uniform density of the mass M, and the radius R, the potential upon itself V—if we call the mass of the earth m, its radius r, and the intensity of gravity at its surface g—has the value

$$V = \frac{3}{5} \cdot \frac{r^2 M^2}{Rm} \cdot g.$$

Let us regard the bodies of our system as such spheres, then the total work of condensation is equal to the sum of all their potentials on themselves. As, however, these potentials for different spheres are to each other as the quantity $\frac{M^2}{R}$, they all vanish in comparison with the sun; even that of the greatest planet, Jupiter, is only about the one hundred-thousandth part of that of the sun; in the calculation, therefore, it is only necessary to introduce the latter.

To elevate the temperature of a mass M of the specific heat σ, t degrees, we need a quantity of heat equal to

$M\sigma t$; this corresponds, when Ag represents the mechanical equivalent of the unit of heat, to the work $AgM\sigma t$. To find the elevation of temperature produced by the condensation of the mass of the sun, let us set

$$AgM\sigma t = V;$$

we have then

$$t = \frac{3}{5} \cdot \frac{r^2 M}{A \,.\, R \,.\, m \,.\, \sigma}.$$

For a mass of water equal to the sun we have $\sigma = 1$; then the calculation with the known values of A, M, R, m, and r, gives

$$t = 28611000° \text{ Cent.}$$

The mass of the sun is 738 times greater than that of all the planets taken together; if, therefore, we desire to make the water mass equal to that of the entire system, we must multiply the value of t by the fraction $\frac{738}{739}$, which makes hardly a sensible alteration in the result.

When a spherical mass of the radius R condenses more and more to the radius R_1, the elevation of temperature thereby produced is

$$\vartheta = \frac{3}{5} \cdot \frac{r^2 M}{A \,.\, m\sigma} \left\{ \frac{1}{R_1} - \frac{1}{R_0} \right\},$$

or

$$= \frac{3}{5} \cdot \frac{r^2 M}{A R_1 m\sigma} \left\{ 1 - \frac{R_1}{R_0} \right\}.$$

Supposing, then, the mass of the planetary system to be at the commencement, not a sphere of infinite radius, but limited, say of the radius of the path of Neptune, which is six thousand times greater than the radius of the sun, the magnitude $\frac{R_1}{R_0}$ will then be equal to $\frac{1}{6000}$, and the above value of t would have to be diminished by this inconsiderable amount.

From the same formula we can deduce that a diminution of $\frac{1}{10000}$ of the radius of the sun would generate work in a water mass equal to the sun, equivalent to 2,861 degrees Centigrade. And as, according to Pouillet, a quantity of heat corresponding to $1\frac{1}{2}$ degree is lost annually in such a mass, the condensation referred to would cover the loss for 2,289 years.

If the sun, as seems probable, be not everywhere of the same density, but is denser at the centre than near the surface, the potential of its mass and the corresponding quantity of heat will be still greater.

Of the now remaining mechanical forces, the *vis viva* of the rotation of the heavenly bodies round their own axes is, in comparison with the other quantities, very small, and may be neglected. The *vis viva* of the motion of revolution round the sun, if μ be the mass of a planet, and ρ its distance from the sun, is

$$L = \frac{gr^2 M\mu}{m}\left\{\frac{1}{R} - \frac{1}{2\rho}\right\}.$$

Omitting the quantity $\frac{1}{2\rho}$ as very small compared with $\frac{1}{R}$, and dividing by the above value of V, we obtain

$$\frac{L}{V} = \frac{5}{3}\frac{\mu}{M}.$$

The mass of all the planets together is $\frac{1}{738}$ of the mass of the sun; hence the value of L for the entire system is

$$L = \frac{1}{453} \cdot V.$$

THE RECENT PROGRESS OF THE THEORY OF VISION.

A COURSE OF LECTURES DELIVERED IN FRANKFORT AND HEIDELBERG, AND REPUBLISHED IN THE PREUSSISCHE JAHRBUCHER, 1868

I. The Eye as an Optical Instrument.

The physiology of the senses is a border land in which the two great divisions of human knowledge, natural and mental science, encroach on one another's domain; in which problems arise which are important for both, and which only the combined labour of both can solve.

No doubt the first concern of physiology is only with material changes in material organs, and that of the special physiology of the senses is with the nerves and their sensations, so far as these are excitations of the nerves. But, in the course of investigation into the functions of the organs of the senses, science cannot avoid also considering the apprehension of external objects, which is the result of these excitations of the nerves, and for the simple reason that the fact of a particular state of mental apprehension often reveals to us a nervous excitation which would otherwise have escaped our notice. On the other hand, apprehension of external objects must always be an act of our power of realization, and must therefore be accompanied by consciousness, for it is a mental function. Indeed the further exact investigation of this process has been pushed, the more it has revealed to us an ever-widening field of such mental functions,

the results of which are involved in those acts of apprehension by the senses which at first sight appear to be most simple and immediate. These concealed functions have been but little discussed, because we are so accustomed to regard the apprehension of any external object as a complete and direct whole, which does not admit of analysis.

It is scarcely necessary for me to remind my present readers of the fundamental importance of this field of inquiry to almost every other department of science. For apprehension by the senses supplies after all, directly or indirectly, the material of all human knowledge, or at least the stimulus necessary to develope every inborn faculty of the mind. It supplies the basis for the whole action of man upon the outer world; and if this stage of mental processes is admitted to be the simplest and lowest of its kind, it is none the less important and interesting. For there is little hope that he who does not begin at the beginning of knowledge will ever arrive at its end.

It is by this path that the art of experiment, which has become so important in natural science, found entrance into the hitherto inaccessible field of mental processes. At first this will be only so far as we are able by experiment to determine the particular sensible impressions which call up one or another conception in our consciousness. But from this first step will follow numerous deductions as to the nature of the mental processes which contribute to the result. I will therefore endeavour to give some account of the results of physiological inquiries so far as they bear on the questions above mentioned.

I am the more desirous of doing so because I have lately completed [1] a complete survey of the field of physiological optics, and am happy to have an opportunity of

[1] Prof. Helmholtz's *Handbook of Physiological Optics* was published at Leipzig in 1867.

putting together in a compendious form the views and deductions on the present subject which might escape notice among the numerous details of a book devoted to the special objects of natural science. I may state that in that work I took great pains to convince myself of the truth of every fact of the slightest importance by personal observation and experiment. There is no longer much controversy on the more important facts of observation, the chief difference of opinion being as to the extent of certain individual differences of apprehension by the senses. During the last few years a great number of distinguished investigators have, under the influence of the rapid progress of ophthalmic medicine, worked at the physiology of vision; and in proportion as the number of observed facts has increased, they have also become more capable of scientific arrangement and explanation. I need not remind those of my readers who are conversant with the subject how much labour must be expended to establish many facts which appear comparatively simple and almost self-evident.

To render what follows understood in all its bearings, I shall first describe the *physical* characters of the eye as an optical instrument; next the *physiological* processes of excitation and conduction in the parts of the nervous system which belong to it; and lastly I shall take up the *psychological* question, how mental apprehensions are produced by the changes which take place in the optic nerve.

The first part of our inquiry, which cannot be passed over because it is the foundation of what follows, will be in great part a repetition of what is already generally known, in order to bring in what is new in its proper place. But it is just this part of the subject which excites so much interest, as the real starting point of

really constitutes the chief advantage of the other optical instruments.

Indeed the peculiar way in which we are a to give our attention to external objects, by only to one thing at a time, and as soon as thi taken in hastening to another, enables the sen to accomplish as much as is necessary; and practically the same advantage as if we enjoye rate view of the whole field of vision at once. fact until we begin to examine our sensations we become aware of the imperfections of ind Whatever we want to see we look at, and see i what we do not look at, we do not as a rul the moment, and so do not notice how im see it.

Indeed, it is only after long practice able to turn our attention to an object in indirect vision (as is necessary for some observations) without looking at it, and into direct view. And it is just as diffi eye on an object for the number of secon produce the phenomenon of an after-im this well defined requires a good deal of p

A great part of the importance of the e of expression depends on the same fact ments of the eyeball—its glances—are a direct signs of the movement of the at movements of the mind, of the person at us.

Just as quickly as the eye turns upwa and from side to side, does the accomm so as to bring the object to which our the moment directed into focus; and t tant objects pass in rapid succession into

a, p. 254.

courtiers by showing them how they might see each other with their heads cut off.

There are, in addition, a number of smaller gaps in the field of vision, in which a small bright point, a fixed star for example, may be lost. These are caused by the blood-vessels of the retina. The vessels run in the front layers, and so cast their shadow on the part of the sensative mosaic which lies behind them. The larger ones shut off the light from reaching the rods and cones altogether, the more slender at least limit its amount.

These splits in the picture presented by the eye may be recognised by making a hole in a card with a fine needle, and looking through it at the sky, moving the card a little from side to side all the time. A still better experiment is to throw sunlight through a small lens upon the white of the eye at the outer angle (temporal canthus), while the globe is turned as much as possible inwards. The shadow of the blood-vessels is then thrown across on to the inner wall of the retina, and we see them as gigantic branching lines, like fig. 32 magnified. These vessels lie in the front layer of the retina itself, and, of course, their shadow can only be seen when it falls on the proper sensitive layer. So that this phenomenon furnishes a proof that the hindmost layer is that which is sensitive to light. And by its help it has become possible actually to measure the distance between the sensitive and the vascular layers of the retina. It is done as follows:—

If the focus of the light thrown on to the white of the eye (the sclerotic) is moved slightly backwards and forwards, the shadow of the blood-vessels and its image in the field of vision will, of course, move also. The extent of these movements can be easily measured, and from these data Heinrich Müller, of Würzburg—whose too early loss to science we still deplore—determined the distance between the two foci, and found it exactly to equal the thickness

thought he had discovered a relation between the refractive and dispersive powers of various transparent materials, from which it followed that no achromatic refraction was possible. Euler,[1] on the other hand, concluded that, since the eye is achromatic, the relation discovered by Newton could not be correct. Reasoning from this assumption, he constructed theoretical rules for making achromatic instruments, and Dolland[2] carried them out. But Dolland himself observed that the eye could not be achromatic, because its construction did not answer to Euler's rules; and at last Fraunhofer[3] actually measured the degree of chromatic aberration of the eye. An eye constructed to bring red light from infinite distance to a focus on the retina can only do the same with violet rays from a distance of two feet. With ordinary light this is not noticed because these extreme colours are the least luminous of all, and so the images they produce are scarcely observed beside the more intense images of the intermediate yellow, green, and blue rays. But the effect is very striking when we isolate the extreme rays of the spectrum by means of violet glass. Glasses coloured with cobalt oxide allow the red and blue rays to pass, but stop the green and yellow ones, that is, the brightest rays of the spectrum. If those of my readers who have eyes of ordinary focal distance will look at lighted street lamps from a distance with this violet glass, they will see a red flame surrounded by a broad bluish violet halo. This is the dispersive image of the flame thrown by its blue and violet light. The phenomenon is a simple and complete proof of the fact of chromatic aberration in the eye.

Now the reason why this defect is so little noticed

[1] Leonard Euler born at Basel, 1707; died at St. Petersburgh, 1783.
[2] John Dolland, F.R.S. born 1706; died in London, 1761.
[3] Joseph Fraunhofer born in Bavaria, 1787; died at Münich, 1826.

under ordinary circumstances, and why it is in fact somewhat less than a glass instrument of the same construction would have, is that the chief refractive medium of the eye is water, which possesses a less dispersive power than glass.[1] Hence it is that the chromatic aberration of the eye, though present, does not materially affect vision with ordinary white illumination.

A second defect which is of great importance in optical instruments of high magnifying power is what is known as spherical aberration. Spherical refracting surfaces approximately unite the rays which proceed from a luminous point into a single focus, only when each ray falls nearly perpendicularly upon the corresponding part of the refracting surface. If all those rays which form the centre of the image are to be exactly united, a lens with other than spherical surfaces must be used, and this cannot be made with sufficient mechanical perfection. Now the eye has its refracting surfaces partly elliptical; and so here again the natural prejudice in its favour led to the erroneous belief that spherical aberration was thus prevented. But this was a still greater blunder. More accurate investigation showed that much greater defects than that of spherical aberration are present in the eye, defects which are easily avoided with a little care in making optical instruments, and compared with which the amount of spherical aberration becomes very unimportant. The careful measurements of the curvature of the cornea, first made by Senff of Dorpat, next, with a better adapted instrument, the writer's ophthalmometer already referred to, and afterwards carried out in numerous cases by Donders, Knapp, and others, have proved that the cornea of most human eyes is not a perfectly sym-

[1] But still the diffraction in the eye is rather greater than an instrument made with water would produce under the same conditions.

metrical curve, but is variously bent in different directions. I have also devised a method of testing the 'centering' of an eye during life, i.e. ascertaining whether the cornea and the crystalline lens are symmetrically placed with regard to their common axis. By this means I discovered in the eyes I examined slight but distinct deviations from accurate centering. The result of these two defects of construction is the condition called *astigmatism*, which is found more or less in most human eyes, and prevents our seeing vertical and horizontal lines at the same distance perfectly clearly at once. If the degree of astigmatism is excessive, it can be obviated by the use of glasses with cylindrical surfaces, a circumstance which has lately much attracted the attention of oculists.

Nor is this all. A refracting surface which is imperfectly elliptical, an ill-centered telescope, does not give a single illuminated point as the image of a star,

Fig. 31.

but, according to the surface and arrangement of the refracting media, elliptic, circular, or linear images. Now the images of an illuminated point, as the human eye brings them to focus, are even more inaccurate: they are irregularly radiated. The reason of this lies in the con-

courtiers by showing them how they might see each other with their heads cut off.

There are, in addition, a number of smaller gaps in the field of vision, in which a small bright point, a fixed star for example, may be lost. These are caused by the blood-vessels of the retina. The vessels run in the front layers, and so cast their shadow on the part of the sensative mosaic which lies behind them. The larger ones shut off the light from reaching the rods and cones altogether, the more slender at least limit its amount.

These splits in the picture presented by the eye may be recognised by making a hole in a card with a fine needle, and looking through it at the sky, moving the card a little from side to side all the time. A still better experiment is to throw sunlight through a small lens upon the white of the eye at the outer angle (temporal canthus), while the globe is turned as much as possible inwards. The shadow of the blood-vessels is then thrown across on to the inner wall of the retina, and we see them as gigantic branching lines, like fig. 32 magnified. These vessels lie in the front layer of the retina itself, and, of course, their shadow can only be seen when it falls on the proper sensitive layer. So that this phenomenon furnishes a proof that the hindmost layer is that which is sensitive to light. And by its help it has become possible actually to measure the distance between the sensitive and the vascular layers of the retina. It is done as follows:—

If the focus of the light thrown on to the white of the eye (the sclerotic) is moved slightly backwards and forwards, the shadow of the blood-vessels and its image in the field of vision will, of course, move also. The extent of these movements can be easily measured, and from these data Heinrich Müller, of Würzburg—whose too early loss to science we still deplore—determined the distance between the two foci, and found it exactly to equal the thickness

is most apparent in the blue and violet rays of the solar spectrum; for there comes in the phenomenon of fluorescence [1] to increase it.

In fact, although the crystalline lens looks so beautifully clear when taken out of the eye of an animal just killed, it is far from optically uniform in structure. It is possible to see the shadows and dark spots within the eye (the so-called 'entoptic objects') by looking at an extensive bright surface—the clear sky, for instance—through a very narrow opening. And these shadows are chiefly due to the fibres and spots in the lens.

There are also a number of minute fibres, corpuscles and folds of membrane, which float in the vitreous humour, and are seen when they come close in front of the retina, even under the ordinary conditions of vision. They are then called *muscæ volitantes*, because when the observer tries to look [2] at them, they naturally move with the movement of the eye. They seem continually to flit away from the point of vision, and thus look like flying insects. These objects are present in everyone's eyes, and usually float in the highest part of the globe of the eye, out of the field of vision, whence on any sudden movement of the eye they are dislodged and swim freely in the vitreous humour. They may occasionally pass in front of the central pit, and so impair sight. It is a

[1] This term is given to the property which certain substances possess of becoming for a time faintly luminous as long as they receive violet and blue light. The bluish tint of a solution of quinine, and the green colour of uranium glass, depend on this property. The fluorescence of the cornea and crystalline lens appears to depend upon the presence in their tissue of a very small quantity of a substance like quinine. For the physiologist this property is most valuable, for by its aid he can see the lens in a living eye by throwing on it a concentrated beam of blue light, and thus ascertain that it is placed close behind the iris, not separated by a large 'posterior chamber,' as was long supposed. But for seeing, the fluorescence of the cornea and lens is simply disadvantageous.

[2] Vide *supra*, p. 213.

remarkable proof of the way in which we observe, or fail to observe, the impressions made on our senses, that these *muscæ volitantes* often appear something quite new and disquieting to persons whose sight is beginning to suffer from any cause; although, of course, there must have been the same conditions long before.

A knowledge of the way in which the eye is developed in man and other vertebrates explains these irregularities in the structure of the lens and the vitreous body. Both are produced by an invagination of the integument of the embryo. A dimple is first formed, this deepens to a round pit, and then expands until its orifice becomes relatively minute, when it is finally closed and the pit becomes completely shut off. The cells of the scarf-skin which line this hollow form the crystalline lens, the true skin beneath them becomes its capsule, and the loose tissue which underlies the skin is developed into the vitreous humour. The mark where the neck of the fossa was sealed is still to be recognised as one of the 'entoptic images' of many adult eyes.

The last defect of the human eye which must be noticed is the existence of certain inequalities of the surface which receives the optical image. Not far from the centre of the field of vision there is a break in the retina, where the optic nerve enters. Here there is nothing but nerve fibres and blood-vessels; and, as the cones are absent, any rays of light which fall on the optic nerve itself are unperceived. This 'blind spot' will therefore produce a corresponding gap in the field of vision where nothing will be visible. Fig. 32 shows the posterior half of the globe of a right eye which has been cut across. R is the retina with its branching blood-vessels. The point from which these diverge is that at which the optic nerve enters. To the reader's left is seen the 'yellow spot.'

Now the gap caused by the presence of the optic nerve is no slight one. It is about 6° in horizontal and 8° in vertical dimension. Its inner border is about 12° horizontally distant from the 'temporal' or external side of the centre of distinct vision. The way to recognise this blind spot most readily is doubtless known to many of my readers. Take a sheet of white paper and mark on it a little cross; then to the right of this, on the same level, and about three inches off, draw a round black spot

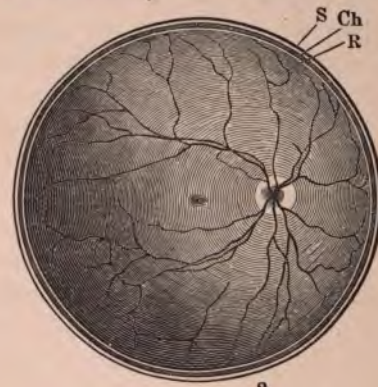


FIG. 32.

half an inch in diameter. Now, holding the paper at arm's length, shut the left eye, fix the right upon the cross, and bring the paper gradually nearer. When it is about eleven inches from the eye, the black spot will suddenly disappear, and will again come into sight as the paper is moved nearer.

This blind spot is so large that it might prevent our seeing eleven full moons if placed side by side, or a man's face at a distance of only six or seven feet. Mariotte,[1] who discovered the phenomenon, amused Charles II. and his

[1] Edme. Mariotte born in Burgundy, died at Paris, 1684.

courtiers by showing them how they might see each other with their heads cut off.

There are, in addition, a number of smaller gaps in the field of vision, in which a small bright point, a fixed star for example, may be lost. These are caused by the blood-vessels of the retina. The vessels run in the front layers, and so cast their shadow on the part of the sensative mosaic which lies behind them. The larger ones shut off the light from reaching the rods and cones altogether, the more slender at least limit its amount.

These splits in the picture presented by the eye may be recognised by making a hole in a card with a fine needle, and looking through it at the sky, moving the card a little from side to side all the time. A still better experiment is to throw sunlight through a small lens upon the white of the eye at the outer angle (temporal canthus), while the globe is turned as much as possible inwards. The shadow of the blood-vessels is then thrown across on to the inner wall of the retina, and we see them as gigantic branching lines, like fig. 32 magnified. These vessels lie in the front layer of the retina itself, and, of course, their shadow can only be seen when it falls on the proper sensitive layer. So that this phenomenon furnishes a proof that the hindmost layer is that which is sensitive to light. And by its help it has become possible actually to measure the distance between the sensitive and the vascular layers of the retina. It is done as follows:—

If the focus of the light thrown on to the white of the eye (the sclerotic) is moved slightly backwards and forwards, the shadow of the blood-vessels and its image in the field of vision will, of course, move also. The extent of these movements can be easily measured, and from these data Heinrich Müller, of Würzburg—whose too early loss to science we still deplore—determined the distance between the two foci, and found it exactly to equal the thickness

which actually separates the layer of rods and cones from the vascular layer of the retina.

The condition of the point of clearest vision (the yellow spot) is disadvantageous in another way. It is less sensitive to weak light than the other parts of the retina. It has been long known that many stars of inferior magnitude—for example, the *Coma Berenicæ* and the Pleiades —are seen more brightly if looked at somewhat obliquely than when their rays fall full upon the eye. This can be proved to depend partly on the yellow colour of the *macula*, which weakens blue more than other rays. It may also be partly the result of the absence of vessels at this yellow spot which has been noticed above, which interferes with its free communication with the life-giving blood.

All these imperfections would be exceedingly troublesome in an artificial camera obscura and in the photographic picture it produced. But they are not so in the eye—so little, indeed, that it was very difficult to discover some of them. The reason of their not interfering with our perception of external objects is not simply that we have two eyes, and so one makes up for the defects of the other. For even when we do not use both, and in the case of persons blind of one eye, the impression we receive from the field of vision is free from the defects which the irregularity of the retina would otherwise occasion. The chief reason is that we are continually moving the eye, and also that the imperfections almost always affect those parts of the field to which we are not at the moment directing our attention.

But, after all it remains a wonderful paradox, that we are so slow to observe these and other peculiarities of vision (such as the after-images of bright objects), so long as they are not strong enough to prevent our seeing

external objects. It is a fact which we constantly meet, not only in optics, but in studying the perceptions produced by other senses on the consciousness. The difficulty with which we perceive the defect of the blind spot is well shown by the history of its discovery. Its existence was first demonstrated by theoretical arguments. While the long controversy whether the perception of light resided in the retina or the choroid was still undecided, Mariotte asked himself what perception there was where the choroid is deficient. He made experiments to ascertain this point, and in the course of them discovered the blind spot. Millions of men had used their eyes for ages, thousands had thought over the nature and cause of their functions, and, after all, it was only by a remarkable combination of circumstances that a simple phenomenon was noticed which would apparently have revealed itself to the slightest observation. Even now, anyone who tries for the first time to repeat the experiment which demonstrates the existence of the blind spot, finds it difficult to divert his attention from the fixed point of clear vision, without losing sight of it in the attempt. Indeed, it is only by long practice in optical experiments that even an experienced observer is able, as soon as he shuts one eye, to recognise the blank space in the field of vision which corresponds to the blind spot.

Other phenomena of this kind have only been discovered by accident, and usually by persons whose senses were peculiarly acute, and whose power of observation was unusually stimulated. Among these may be mentioned Goethe, Purkinje,[1] and Johannes Müller.[2] When a sub-

[1] A distinguished embryologist, for many years professor at Breslau: he died at Prague, 1869, æt. 82.

[2] A great biologist, in the full sense of the term. He was professor of physiology at Berlin, and died 1858, æt. 57. His *Manual of Physiology* was translated into English by the late Dr. Baly.—Tr.

sequent observer tries to repeat on his own eyes these experiments as he finds them described, it is of course easier for him than for the discoverer; but even now there are many of the phenomena described by Purkinje which have never been seen by anyone else, although it cannot be certainly held that they depended on individual peculiarities of this acute observer's eyes.

The phenomena of which we have spoken, and a number of others also, may be explained by the general rule that it is much easier to recognise any change in the condition of a nerve than a constant and equable impression on it. In accordance with this rule, all peculiarities in the excitation of separate nerve fibres, which are equally present during the whole of life (such as the shadow of the blood-vessels of the eye, the yellow colour of the central pit of the retina, and most of the fixed entoptic images), are never noticed at all; and if we want to observe them we must employ unusual modes of illumination and, particularly, constant change of its direction.

According to our present knowledge of the conditions of nervous excitation, it seems to me to be very unlikely that we have here to do with a simple property of sensation; it must, I think, be rather explained as a phenomenon belonging to our power of attention, and I now only refer to the question in passing, since its full discussion will come afterwards in its proper connection.

So much for the physical properties of the Eye. If I am asked why I have spent so much time in explaining its imperfection to my readers, I answer, as I said at first, that I have not done so in order to depreciate the performances of this wonderful organ or to diminish our admiration of its construction. It was my object to make the reader understand, at the first step of our inquiry, that it

is not any mechanical perfection of the organs of our senses which secures for us such wonderfully true and exact impressions of the outer world. The next section of this inquiry will introduce much bolder and more paradoxical conclusions than any I have yet stated. We have now seen that the eye in itself is not by any means so complete an optical instrument as it first appears: its extraordinary value depends upon the way in which we use it: its perfection is practical, not absolute, consisting not in the avoidance of every error, but in the fact that all its defects do not prevent its rendering us the most important and varied services.

From this point of view, the study of the eye gives us a deep insight into the true character of organic adaptation generally. And this consideration becomes still more interesting when brought into relation with the great and daring conceptions which Darwin has introduced into science, as to the means by which the progressive perfection of the races of animals and plants has been carried on. Wherever we scrutinise the construction of physiological organs, we find the same character of practical adaptation to the wants of the organism; although, perhaps, there is no instance which we can follow out so minutely as that of the eye.

For the eye has every possible defect that can be found in an optical instrument, and even some which are peculiar to itself; but they are all so counteracted, that the inexactness of the image which results from their presence very little exceeds, under ordinary conditions of illumination, the limits which are set to the delicacy of sensation by the dimensions of the retinal cones. But as soon as we make our observations under somewhat changed conditions, we become aware of the chromatic aberration, the astigmatism, the blind spots, the venous shadows, the

imperfect transparency of the media, and all the other defects of which I have spoken.

The adaptation of the eye to its function is, therefore, most complete, and is seen in the very limits which are set to its defects. Here the result which may be reached by innumerable generations working under the Darwinian law of inheritance, coincides with what the wisest Wisdom may have devised beforehand. A sensible man will not cut firewood with a razor, and so we may assume that each step in the elaboration of the eye must have made the organ more vulnerable and more slow in its development. We must also bear in mind that soft, watery animal textures must always be unfavourable and difficult material for an instrument of the mind.

One result of this mode of construction of the eye, of which we shall see the importance bye and bye, is that clear and complete apprehension of external objects by the sense of sight is only possible when we direct our attention to one part after another of the field of vision in the manner partly described above. Other conditions, which tend to produce the same limitation, will afterwards come under our notice.

But, apparently, we are not yet come much nearer to understanding sight. We have only made one step: we have learnt how the optical arrangement of the eye renders it possible to separate the rays of light which come in from all parts of the field of vision, and to bring together again all those that have proceeded from a single point, so that they may produce their effect upon a single fibre of the optic nerve.

Let us see, therefore, how much we know of the sensations of the eye, and how far this will bring us towards the solution of the problem.

II. The Sensation of Sight.

In the first section of our subject we have followed the course of the rays of light as far as the retina, and seen what is the result produced by the peculiar arrangement of the optical apparatus. The light which is reflected from the separate illuminated points of external objects is again united in the sensitive terminal structures of separate nerve fibres, and thus throws them into action without affecting their neighbours. At this point the older physiologists thought they had solved the problem, so far as it appeared to them to be capable of solution. External light fell directly upon a sensitive nervous structure in the retina, and was, as it seemed, directly felt there.

But during the last century, and still more during the first quarter of this, our knowledge of the processes which take place in the nervous system was so far developed, that Johannes Müller, as early as the year 1826,[1] when writing that great work on the 'Comparative Physiology of Vision,' which marks an epoch in science, was able to lay down the most important principles of the theory of the impressions derived from the senses. These principles have not only been confirmed in all important points by subsequent investigation, but have proved of even more extensive application than this eminent physiologist could have suspected.

The conclusions which he arrived at are generally comprehended under the name of the theory of the Specific

[1] The year in which he was appointed Extraordinary Professor of Physiology in the University of Bonn.

Action of the Senses. They are no longer so novel that they can be reckoned among the latest advances of the theory of vision, which form the subject of the present essay. Moreover, they have been frequently expounded in a popular form by others as well as by myself.[1] But that part of the theory of vision with which we are now occupied is little more than a further development of the theory of the specific action of the senses. I must, therefore, beg my reader to forgive me if, in order to give him a comprehensive view of the whole subject in its proper connection, I bring before him much which he already knows, while I also introduce the more recent additions to our knowledge in their appropriate places.

All that we apprehend of the external world is brought to our consciousness by means of certain changes which are produced in our organs of sense by external impressions, and transmitted to the brain by the nerves. It is in the brain that these impressions first become conscious sensations, and are combined so as to produce our conceptions of surrounding objects. If the nerves which convey these impressions to the brain are cut through, the sensation, and the perception of the impression, immediately cease. In the case of the eye, the proof that visual perception is not produced directly in each retina, but only in the brain itself by means of the impressions transmitted to it from both eyes, lies in the fact (which I shall afterwards more fully explain) that the visual impression of any solid object of three dimensions is only produced by the combination of the impressions derived from both eyes.

What, therefore, we directly apprehend is not the immediate action of the external exciting cause upon the ends

[1] 'On the Nature of Special Sensations in Man,' *Königsberger naturwissenschaftliche Unterhaltungen*, vol. iii. 1852. 'Human Vision,' a popular Scientific Lecture by H. Helmholtz, Leipzig, 1855.

of our nerves, but only the changed condition of the nervous fibres which we call the state of *excitation* or functional activity.

Now all the nerves of the body, so far as we at present know, have the same structure, and the change which we call excitation is in each of them a process of precisely the same kind, whatever be the function it subserves. For while the task of some nerves is that already mentioned, of carrying sensitive impressions from the external organs to the brain, others convey voluntary impulses in the opposite direction, from the brain to the muscles, causing them to contract, and so moving the limbs. Other nerves, again, carry an impression from the brain to certain glands, and call forth their secretion, or to the heart and to the blood-vessels, and regulate the circulation. But the fibres of all these nerves are the same clear, cylindrical threads of microscopic minuteness, containing the same oily and albuminous material. It is true that there is a difference in the diameter of the fibres, but this, so far as we know, depends only upon minor causes, such as the necessity of a certain strength and of getting room for a certain number of independent conducting fibres. It appears to have no relation to their peculiarities of function.

Moreover, all nerves have the same electro-motor actions, as the researches of Du Bois Reymond [1] prove. In all of them the condition of excitation is called forth by the same mechanical, electrical, chemical, or thermometric changes. It is propagated with the same rapidity, of about one hundred feet in the second, to each end of the fibres, and produces the same changes in their electromotor properties. Lastly, all nerves die when submitted to like conditions, and, with a slight apparent dif-

[1] Professor of Physiology in the University of Berlin.

ference according to their thickness, undergo the same coagulation of their contents. In short, all that we can ascertain of nervous structure and function, apart from the action of the other organs with which they are united and in which during life we see the proofs of their activity, is precisely the same for all the different kinds of nerves. Very lately the French physiologists, Philippeau and Vulpian, after dividing the motor and sensitive nerves of the tongue, succeeded in getting the upper half of the sensitive nerve to unite with the lower half of the motor. After the wound had healed, they found that irritation of the upper half, which in normal conditions would have been felt as a sensation, now excited the motor branches below, and thus caused the muscles of the tongue to move. We conclude from these facts that all the difference which is seen in the excitation of different nerves depends only upon the difference of the organs to which the nerve is united, and to which it transmits the state of excitation.

The nerve-fibres have been often compared with telegraphic wires traversing a country, and the comparison is well fitted to illustrate this striking and important peculiarity of their mode of action. In the net-work of telegraphs we find everywhere the same copper or iron wires carrying the same kind of movement, a stream of electricity, but producing the most different results in the various stations according to the auxiliary apparatus with which they are connected. At one station the effect is the ringing of a bell, at another a signal is moved, and at a third a recording instrument is set to work. Chemical decompositions may be produced which will serve to spell out the messages, and even the human arm may be moved by electricity so as to convey telegraphic signals. When the Atlantic cable was being laid, Sir William Thomson found that the slightest signals could be recognised by the

sense of taste, if the wire was laid upon the tongue. Or, again, a strong electric current may be transmitted by telegraphic wires in order to ignite gunpowder for blasting rocks. In short, everyone of the hundred different actions which electricity is capable of producing may be called forth by a telegraphic wire laid to whatever spot we please, and it is always the same process in the wire itself which leads to these diverse consequences. Nerve-fibres and telegraphic wires are equally striking examples to illustrate the doctrine that the same causes may, under different conditions, produce different results. However commonplace this may now sound, mankind had to work long and hard before it was understood, and before this doctrine replaced the belief previously held in the constant and exact correspondence between cause and effect. And we can scarcely say that the truth is even yet universally recognised, since in our present subject its consequences have been till lately disputed.

Therefore, as motor nerves, when irritated, produce movement, because they are connected with muscles, and glandular nerves secretion, because they lead to glands, so do sensitive nerves, when they are irritated, produce sensation, because they are connected with sensitive organs. But we have very different kinds of sensation. In the first place, the impressions derived from external objects fall into five groups, entirely distinct from each other. These correspond to the five senses, and their difference is so great that it is not possible to compare in quality a sensation of light with one of sound or of smell. We will name this difference, so much deeper than that between comparable qualities, a difference of the mode, or *kind*, of sensation, and will describe the differences between impressions belonging to the same sense (for example, the difference between the various sensations of colour) as a difference of *quality*.

Whether by the irritation of a nerve we produce a muscular movement, a secretion or a sensation depends upon whether we are handling a motor, a glandular, or a sensitive nerve, and not at all upon what means of irritation we may use. It may be an electrical shock, or tearing the nerve, or cutting it through, or moistening it with a solution of salt, or touching it with a hot wire. In the same way (and this great step in advance was due to Johannes Müller) the *kind* of sensation which will ensue when we irritate a sensitive nerve, whether an impression of light, or of sound, or of feeling, or of smell, or of taste, will be produced, depends entirely upon which sense the excited nerve subserves, and not at all upon the method of excitation we adopt.

Let us now apply this to the optic nerve, which is the object of our present enquiry. In the first place, we know that no kind of action upon any part of the body except the eye and the nerve which belongs to it, can ever produce the sensation of light. The stories of somnambulists, which are the only arguments that can be adduced against this belief, we may be allowed to disbelieve. But, on the other hand, it is not light alone which can produce the sensation of light upon the eye, but also any other power which can excite the optic nerve. If the weakest electrical currents are passed through the eye they produce flashes of light. A blow, or even a slight pressure made upon the side of the eyeball with the finger, makes an impression of light in the darkest room, and, under favourable circumstances, this may become intense. In these cases it is important to remember that there is no objective light produced in the retina, as some of the older physiologists assumed, for the sensation of light may be so strong that a second observer could not fail to see through the pupil the illumination of the retina which would follow, if the

sensation were really produced by an actual development of light within the eye. But nothing of the sort has ever been seen. Pressure or the electric current excites the optic nerve, and therefore, according to Müller's law, a sensation of light results, but under these circumstances, at least, there is not the smallest spark of actual light.

In the same way, increased pressure of blood, its abnormal constitution in fevers, or its contamination with intoxicating or narcotic drugs, can produce sensations of light to which no actual light corresponds. Even in cases in which an eye is entirely lost by accident or by an operation, the irritation of the stump of the optic nerve while it is healing is capable of producing similar subjective effects. It follows from these facts that the peculiarity in kind which distinguishes the sensation of light from all others does not depend upon any peculiar qualities of light itself. Every action which is capable of exciting the optic nerve is capable of producing the impression of light; and the purely subjective sensation thus produced is so precisely similar to that caused by external light, that persons unacquainted with these phenomena readily suppose that the rays they see are real objective beams.

Thus we see that external light produces no other effects in the optic nerve than other agents of an entirely different nature. In one respect only does light differ from the other causes which are capable of exciting this nerve: namely, that the retina, being placed at the back of the firm globe of the eye, and further protected by the bony orbit, is almost entirely withdrawn from other exciting agents, and is thus only exceptionally affected by them, while it is continually receiving the rays of light which stream in upon it through the transparent media of the eye.

On the other hand, the optic nerve, by reason of the peculiar structures in connection with the ends of its fibres, the rods and cones of the retina, is incomparably more sensitive to rays of light than any other nervous apparatus of the body, since the rest can only be affected by rays which are concentrated enough to produce noticeable elevation of temperature.

This explains why the sensations of the optic nerve are for us the ordinary sensible sign of the presence of light in the field of vision, and why we always connect the sensation of light with light itself, even where they are really unconnected. But we must never forget that a survey of all the facts in their natural connection puts it beyond doubt that external light is only *one* of the exciting causes capable of bringing the optic nerve into functional activity, and therefore that there is no exclusive relation between the sensation of light and light itself.

Now that we have considered the action of excitants upon the optic nerve in general, we will proceed to the qualitative differences of the sensation of light, that is to say, to the various sensations of colour. We will try to ascertain how far these differences of sensation correspond to actual differences in external objects.

Light is known in Physics as a movement which is propagated by successive waves in the elastic ether distributed through the universe, a movement of the same kind as the circles which spread upon the smooth surface of a pond when a stone falls on it, or the vibration which is transmitted through our atmosphere as sound. The chief difference is, that the rate with which light spreads, and the rapidity of movement of the minute particles which form the waves of ether, are both enormously greater than that of the waves of water or of air. The waves of light sent forth from the sun differ exceedingly in size, just as

the little ripples whose summits are a few inches distant from each other differ from the waves of the ocean, between whose foaming crests lie valleys of sixty or a hundred feet. But, just as high and low, short and long waves, on the surface of water, do not differ in kind, but only in size, so the various waves of light which stream from the sun differ in their height and length, but move all in the same manner, and show (with certain differences depending upon the length of the waves) the same remarkable properties of reflection, refraction, interference, diffraction, and polarisation. Hence we conclude that the undulating movement of the ether is in all of them the same. We must particularly note that the phenomena of interference, under which light is now strengthened, and now obscured by light of the same kind, according to the distance it has traversed, prove that all the rays of light depend upon oscillations of waves; and further, that the phenomena of polarisation, which differ according to different lateral directions of the rays, show that the particles of ether vibrate at right angles to the direction in which the ray is propagated.

All the different sorts of rays which I have mentioned produce one effect in common. They raise the temperature of the objects on which they fall, and accordingly are all felt by our skin as rays of heat.

On the other hand, the eye only perceives one part of these vibrations of ether as light. It is not at all cognisant of the waves of great length, which I have compared with those of the ocean; these, therefore, are named the *dark heat-rays*. Such are those which proceed from a warm but not red-hot stove, and which we recognise as heat, but not as light.

Again, the waves of shortest length, which correspond with the very smallest ripples produced by a gentle breeze, are so slightly appreciated by the eye, that such rays are

also generally regarded as invisible, and are known as the *dark chemical rays*.

Between the very long and the very short waves of ether there are waves of intermediate length, which strongly affect the eye, but do not essentially differ in any other physical property from the dark rays of heat and the dark chemical rays. The distinction between the visible and invisible rays depends only on the different length of their waves and the different physical relations which result therefrom. We call these middle rays Light, because they alone illuminate our eyes.

When we consider the heating property of these rays we also call them luminous heat; and because they produce such a very different impression on our skin and on our eyes, heat was universally considered as an entirely different kind of radiation from light, until about thirty years ago. But both kinds of radiation are inseparable from one another in the illuminating rays of the sun; indeed, the most careful recent investigations prove that they are precisely identical. To whatever optical processes they may be subjected, it is impossible to weaken their illuminating power without at the same time, and in the same degree, diminishing their heating and their chemical action. Whatever produces an undulatory movement of ether, of course produces thereby all the effects of the undulation, whether light, or heat, or fluorescence, or chemical change.

Those undulations which strongly affect our eyes, and which we call light, excite the impression of different colours, according to the length of the waves. The undulations with the longest waves appear to us red; and as the length of the waves gradually diminishes they seem to be golden-yellow, yellow, green, blue, violet, the last colour being that of the illuminating rays which

have the smallest wave-length. This series of colours is universally known in the rainbow. We also see it if we look towards the light through a glass prism, and a diamond sparkles with hues which follow in the same order. In passing through transparent prisms, the primitive beam of white light, which consists of a multitude of rays of various colour and various wave-length, is decomposed by the different degree of refraction of its several parts, referred to in the last essay; and thus each of its component hues appears separately. These colours of the several primary forms of light are best seen in the spectrum produced by a narrow streak of light passing through a glass prism: they are at once the fullest and the most brilliant which the external world can show.

When several of these colours are mixed together, they give the impression of a new colour, which generally seems more or less white. If they were all mingled in precisely the same proportions in which they are combined in the sun-light, they would give the impression of perfect white. According as the rays of greatest, middle, or least wave-length predominate in such a mixture, it appears as reddish-white, greenish-white, bluish-white, and so on.

Everyone who has watched a painter at work knows that two colours mixed together give a new one. Now, although the results of the mixture of coloured light differ in many particulars from those of the mixture of pigments, yet on the whole the appearance to the eye is similar in both cases. If we allow two different coloured lights to fall at the same time upon a white screen, or upon the same part of our retina, we see only a single compound colour, more or less different from the two original ones.

The most striking difference between the mixture of pigments and that of coloured light is, that while

painters make green by mixing blue and yellow pigments, the union of blue and yellow rays of light produces white. The simplest way of mixing coloured light is shown in Fig. 33. *P* is a small flat piece of glass; *b* and *g* are two coloured wafers. The observer looks at *b* through the glass plate, while *g* is seen reflected in the same; and if *g* is put in a proper position, its image exactly coincides with that of *b*. It then appears as if there was a single wafer at *b*, with a colour produced by the mixture of the two real ones. In this experiment the light from *b*, which traverses the glass pane, actually unites with that from *g*, which is reflected from it, and the two combined pass on to the retina at *o*. In general, then, light, which consists of undulations of different wave-lengths, produces different impressions upon our eye, namely, those of different colours. But the number of hues which we can recognise is much smaller than that of the various possible combinations of rays with different wave-lengths which external objects can convey to our eyes. The retina cannot distinguish between the white which is produced by the union of scarlet and bluish-green light, and that which is composed of yellowish-green and violet, or of yellow and ultramarine blue, or of red, green, and violet, or of all the colours of the spectrum united. All these combinations appear identically as white; and yet, from a physical point of view, they are very different. In fact, the only resemblance between the several combinations just mentioned is, that they are indistinguishable to the human eye. For instance, a surface illuminated with red and bluish-green light would come out black in a photograph; while another lighted

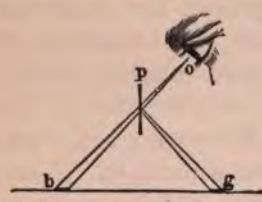


FIG. 33.

with yellowish green and violet would appear very bright, although both surfaces alike seem to the eye to be simply white. Again, if we successively illuminate coloured objects with white beams of light of various composition, they will appear differently coloured. And whenever we decompose two such beams by a prism, or look at them through a coloured glass, the difference between them at once becomes evident.

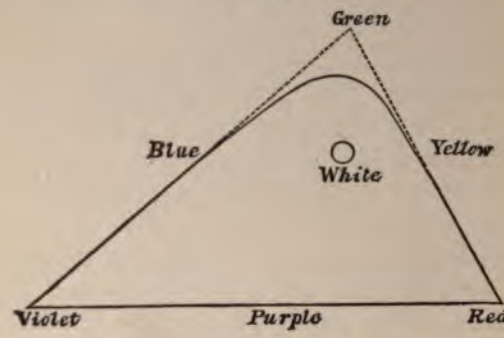
Other colours, also, especially when they are not strongly pronounced, may, like pure white light, be composed of very different mixtures, and yet appear indistinguishable to the eye, while in every other property, physical or chemical, they are entirely distinct.

Newton first showed how to represent the system of colours distinguishable to the eye in a simple diagrammatic form; and by the same means it is comparatively easy to demonstrate the law of the combination of colours. The primary colours of the spectrum are arranged in a series around the circumference of a circle, beginning with red, and by imperceptible degrees passing through the various hues of the rainbow to violet. The red and violet are united by shades of purple, which on the one side pass off to the indigo and blue tints, and on the other through crimson and scarlet to orange. The middle of the circle is left white, and on lines which run from the centre to the circumference are represented the various tints which can be produced by diluting the full colours of the circumference until they pass into white. A colour-disc of this kind shows all the varieties of hue which can be produced with the same amount of light.

It will now be found possible so to arrange the places of the several colours in this diagram, and the quantity of light which each reflects, that when we have ascertained the resultants of two colours of different known

strength of light (in the same way as we might determine the centre of gravity of two bodies of different known weights), we shall then find their combination-colour at the 'centre of gravity' of the two amounts of light.

FIG. 34.



That is to say, that in a properly constructed colour-disc, the combination-colour of any two colours will be found upon a straight line drawn from between them; and compound colours which contain more of one than of the other component hue, will be found in that proportion nearer to the former, and further from the latter.

We find, however, when we have drawn our diagram, that those colours of the spectrum which are most saturated in nature, and which must therefore be placed at the greatest distance from the central white, will not arrange themselves in the form of a circle. The circumference of the diagram presents three projections corresponding to the red, the green, and the violet, so that the colour circle is more properly a triangle, with the corners rounded off, as seen in Fig. 34. The continuous line represents the curve of the colours of the spectrum, and the small circle in the middle the white. At the cor-

ners are the three colours I have mentioned,[1] and the sides of the triangle show the transitions from red through yellow into green, from green through bluish-green and ultramarine to violet, and from violet through purple to scarlet.

Newton used the diagram of the colours of the spectrum (in a somewhat different form from that just given) only as a convenient way of representing the facts to the eye; but recently Maxwell has succeeded in demonstrating the strict and even quantitative accuracy of the principles involved in the construction of this diagram. His method is to produce combinations of colours on swiftly rotating discs, painted of various tints in sectors. When such a disc is turned rapidly round, so that the eye can no longer follow the separate hues, they melt into a uniform combination-colour, and the quantity of light which belongs to each can be directly measured by the breadth of the sector of the circle it occupies. Now the combination-colours which are produced in this manner are exactly those which would result if the same qualities of coloured light illuminated the same surface continuously, as can be experimentally proved. Thus have the relations of size and number been introduced into the apparently inaccessible region of colours, and their differences in quality have been reduced to relations of quantity.

All differences between colours may be reduced to three, which may be described as difference of tone, difference of fulness, or, as it is technically called, 'saturation,' and difference of brightness. The differences of *tone* are those which exist between the several colours of the spectrum, and to which we give the names red, yellow, blue, violet, purple. Thus, with regard to tone, colours form a series

[1] The author has restored violet as a primitive colour in accordance with the experiments of J. J. Müller, having in the first edition adopted the opinion of Maxwell that it is blue.

which returns upon itself; a series which we complete when we allow the terminal colours of the rainbow to pass into one another through purple and crimson. It is in fact the same which we described as arranged around the circumference of the colour-disc.

The *fulness* or saturation of colours is greatest in the pure tints of the spectrum, and becomes less in proportion as they are mixed with white light. This, at least, is true for colours produced by external light, but for our sensations it is possible to increase still further the apparent saturation of colour, as we shall presently see. Pink is a whitish-crimson, flesh-colour a whitish-scarlet, and so pale green, straw-colour, light blue, &c., are all produced by diluting the corresponding colours with white. All compound colours are, as a rule, less saturated than the simple tints of the spectrum.

Lastly, we have the difference of *brightness*, or strength of light, which is not represented in the colour-disc. As long as we observe coloured rays of light, difference in brightness appears to be only one of quantity, not of quality. Black is only darkness—that is, simple absence of light. But when we examine the colours of external objects, black corresponds just as much to a peculiarity of surface in reflection, as does white, and therefore has as good a right to be called a colour. And as a matter of fact, we find in common language a series of terms to express colours with a small amount of light. We call them *dark* (or rather in English, *deep*) when they have little light but are 'full' in tint, and *grey* when they are 'pale.' Thus dark blue conveys the idea of depth in tint, of a full blue with a small amount of light; while grey-blue is a pale blue with a small amount of light. In the same way, the colours known as maroon, brown and olive are dark, more or less saturated tints of red, yellow and green respectively.

In this way we may reduce all possible actual (objective) differences in colour, so far as they are appreciated by the eye, to three kinds; difference of hue (*tone*), difference of fulness (*saturation*), and difference of amount of illumination (*brightness*). It is in this way that we describe the system of colours in ordinary language. But we are able to express this threefold difference in another way.

I said above that a properly constructed colour-disc approaches a triangle in its outline. Let us suppose for a moment that it is an exact rectilinear triangle, as made by the dotted line in Fig. 34; how far this differs from the actual condition we shall have afterwards to point out. Let the colours red, green, and violet be placed at the corners, then we see the law which was mentioned above: namely, that all the colours in the interior and on the sides of the triangle are compounds of the three at its corners. It follows that *all differences of hue depend upon combinations in different proportions of the three primary colours*. It is best to consider the three just named as primary; the old ones red, yellow, and blue are inconvenient, and were only chosen from experience of painters' colours. It is impossible to make a green out of blue and yellow light.

We shall better understand the remarkable fact that we are able to refer all the varieties in the composition of external light to mixtures of three primitive colours, if in this respect we compare the eye with the ear.

Sound, as I mentioned before, is, like light, an undulating movement, spreading by waves. In the case of sound also, we have to distinguish waves of various length which produce upon our ear impressions of different quality. We recognise the long waves as low notes, the short as high-pitched, and the ear may receive at once many waves of sound—that is to say, many notes. But here these do not melt into compound notes, in the same way

that colours, when perceived at the same time and place, melt into compound colours. The eye cannot tell the difference, if we substitute orange for red and yellow; but if we hear the notes C and E sounded at the same time, we cannot put D instead of them, without entirely changing the impression upon the ear. The most complicated harmony of a full orchestra becomes changed to our perception if we alter any one of its notes. No accord (or consonance of several tones) is, at least for the practised ear, completely like another, composed of different tones; whereas, if the ear perceived musical tones as the eye colours, every accord might be completely represented by combining only three constant notes, one very low, one very high, and one intermediate, simply changing the relative strength of these three primary notes to produce all possible musical effects.

In reality we find that an accord only remains unchanged to the ear, when the strength of each separate tone which it contains remains unchanged. Accordingly, if we wish to describe it exactly and completely, the strength of each of its component tones must be exactly stated.

In the same way, the physical nature of a particular kind of light can only be fully ascertained by measuring and noting the amount of light of each of the simple colours which it contains. But in sunlight, in the light of most of the stars, and in flames, we find a continuous transition of colours into one another through numberless intermediate gradations. Accordingly, we must ascertain the amount of light of an infinite number of compound rays if we would arrive at an exact physical knowledge of sun or starlight. In the sensations of the eye we need distinguish for this purpose only the varying intensities of three components.

The practised musician is able to catch the separate *notes* of the various instruments among the complicated

harmonies of an entire orchestra, but the optician cannot directly ascertain the composition of light by means of the eye; he must make use of the prism to decompose the light for him. As soon, however, as this is done, the composite character of light becomes apparent, and he can then distinguish the light of separate fixed stars from one another by the dark and bright lines which the spectrum shows him, and can recognise what chemical elements are contained in flames which are met with on the earth, or even in the intense heat of the sun's atmosphere, in the fixed stars, or in the nebulæ. The fact that light derived from each separate source carries with it certain permanent physical peculiarities is the foundation of spectral analysis—that most brilliant discovery of recent years, which has opened the extreme limits of celestial space to chemical analysis.

There is an extremely interesting and not very uncommon defect of sight which is known as colour-blindness. In this condition the differences of colour are reduced to a still more simple system than that described above; namely, to combinations of only two primary colours. Persons so affected are called *colour-blind*, because they confound certain hues which appear very different to ordinary eyes. At the same time they distinguish other colours, and that quite as accurately, or even (as it seems) rather more accurately, than ordinary people. They are usually 'red-blind'; that is to say, there is no red in their system of colours, and accordingly they see no difference which is produced by the addition of red. All tints are for them varieties of blue and green or, as they call it, yellow. Accordingly scarlet, flesh-colour, white, and bluish-green appear to them to be identical, or at the utmost to differ in brightness. The same applies to crimson, violet, and blue, and to red, orange, yellow, and green. The scarlet flowers of the

geranium have for them exactly the same colours as its leaves. They cannot distinguish between the red and the green signals of trains. They cannot see the red end of the spectrum at all. Very full scarlet appears to them almost black, so that a red-blind Scotch clergyman went to buy scarlet cloth for his gown, thinking it was black.[1]

In this particular of discrimination of colours, we find remarkable inequalities in different parts of the retina. In the first place all of us are red-blind in the outermost part of our field of vision. A geranium-blossom when moved backwards and forwards just within the field of sight, is only recognised as a moving object. Its colour is not seen, so that if it is waved in front of a mass of leaves of the same plant it cannot be distinguished from them in hue. In fact, all red colours appear much darker when viewed indirectly. This red-blind part of the retina is most extensive on the inner or nasal side of the field of vision; and according to recent researches of Woinow, there is at the furthest limit of the visible field a narrow zone in which all distinction of colours ceases and there only remain differences of brightness. In this outermost circle everything appears white, grey, or black. Probably those nervous fibres which convey impressions of green light are alone present in this part of the retina.

In the second place, as I have already mentioned, the middle of the retina, just around the central pit, is coloured yellow. This makes all blue light appear somewhat darker in the centre of the field of sight. The effect is particularly striking with mixtures of red and greenish-blue, which appear white when looked at directly, but acquire a blue tint when viewed at a slight distance

[1] A similar story is told of Dalton, the author of the 'Atomic Theory.' He was a Quaker, and went to the Friends' Meeting, at Manchester, in a pair of scarlet stockings, which some wag had put in place of his ordinary dark grey ones.—Tr.

from the middle of the field; and, on the other hand, when they appear white here, are red to direct vision. These inequalities of the retina, like the others mentioned in the former essay, are rectified by the constant movements of the eye. We know from the pale and indistinct colours of the external world as usually seen, what impressions of indirect vision correspond to those of direct; and we thus learn to judge of the colours of objects according to the impression which they *would* make on us if seen directly. The result is, that only unusual combinations and unusual or special direction of attention enable us to recognise the difference of which I have been speaking.

The theory of colours, with all these marvellous and complicated relations, was a riddle which Goethe in vain attempted to solve; nor were we physicists and physiologists more successful. I include myself in the number; for I long toiled at the task, without getting any nearer my object, until I at last discovered that a wonderfully simple solution had been discovered at the beginning of this century, and had been in print ever since for any one to read who chose. This solution was found and published by the same Thomas Young[1] who first showed the right method of arriving at the interpretation of Egyptian hieroglyphics. He was one of the most acute men who ever lived, but had the misfortune to be too far in advance of his contemporaries. They looked on him with astonishment, but could not follow his bold speculations, and thus a mass of his most important thoughts remained buried and forgotten in the 'Transactions of the Royal Society,' until a later generation by slow degrees arrived at the rediscovery of his discoveries, and came to appreciate the force of his arguments and the accuracy of his conclusions.

[1] Born at Milverton, in Somersetshire, 1773, died 1829.

sation. Just as the difference of sensation of light and warmth depends demonstrably upon whether the rays of the sun fall upon nerves of sight or nerves of feeling, so is supposed in Young's hypothesis that the difference of sensation of colours depends simply upon whether one or the other kind of nervous fibres are more strongly affected. When all three kinds are equally excited, the result is the sensation of white light.

The phenomena that occur in red-blindness must be referred to a condition in which the one kind of nerves, which are sensitive to red rays, are incapable of excitation. It is possible that this class of fibres are wanting, or at least very sparingly distributed, along the edge of the retina, even in the normal human eye.

It must be confessed that both in men and in quadrupeds we have at present no anatomical basis for this theory of colours; but Max Schultze has discovered a structure in birds and reptiles which manifestly corresponds with what we should expect to find. In the eyes of many of this group of animals there are found among the rods of the retina a number which contain a red drop of oil in their anterior end, that namely which is turned towards the light; while other rods contain a yellow drop, and others none at all. Now there can be no doubt that red light will reach the rods with a red drop much better than light of any other colour, while yellow and green light, on the contrary, will find easiest entrance to the rods with the yellow drop. Blue light would be shut off almost completely from both, but would affect the colourless rods all the more effectually. We may therefore with great probability regard these rods as the terminal organs of those nervous fibres which respectively convey impressions of red, of yellow, and of blue light.

I have myself subsequently found a similar hypothesis very convenient and well fitted to explain in a most

simple manner certain peculiarities which have been observed in the perception of musical notes, peculiarities as enigmatical as those we have been considering in the eye. In the *cochlea* of the internal ear the ends of the nerve fibres lie regularly spread out side by side, and provided with minute elastic appendages (the rods of Corti) arranged like the keys and hammers of a piano. My hypothesis is, that here each separate nerve fibre is constructed so as to take cognizance of a definite note, to which its elastic fibre vibrates in perfect consonance. This is not the place to describe the special characters of our sensations of musical tones which led me to frame this hypothesis. Its analogy with Young's theory of colours is obvious, and it refers the origin of overtones, the perception of the quality of sounds, the difference between consonance and dissonance, the formation of the musical scale and other acoustic phenomena to as simple a principle as that of Young. But in the case of the ear, I could point to a much more distinct anatomical foundation for such a hypothesis, and since that time, I have been able actually to demonstrate the relation supposed; not, it is true, in man or any vertebrate animals, whose labyrinth lies too deep for experiment, but in some of the marine Crustacea. These animals have external appendages to their organs of hearing which may be observed in the living animal, jointed filaments to which the fibres of the auditory nerve are distributed; and Hensen, of Kiel, has satisfied himself that some of these filaments are set in motion by certain notes, and others by different ones.

It remains to reply to an objection against Young's theory of colour. I mentioned above that the outline of the colour-disc, which marks the position of the most saturated colours (those of the spectrum), approaches to a triangle in form; but our conclusions upon the theory of

he three primary colours depend upon a perfect rectilinear triangle inclosing the complete colour-system, for only in that case is it possible to produce all possible tints by various combinations of the three primary colours at the angles. It must, however, be remembered that the colour-disc only includes the entire series of colours which actually occur in nature, while our theory has to do with the analysis of our subjective sensations of colour. We need then only assume that actual coloured light does not produce sensations of absolutely pure colour; that red, for instance, even when completely freed from all admixture of white light, still does not excite those nervous fibres alone which are sensitive to impressions of red, but also, to a very slight degree, those which are sensitive to green, and perhaps to a still smaller extent those which are sensitive to violet rays. If this be so, then the sensation which the purest red light produces in the eye is still not the purest sensation of red which we can conceive of as possible. This sensation could only be called forth by a fuller, purer, more saturated red than has ever been seen in this world.

It is possible to verify this conclusion. We are able to produce artificially a sensation of the kind I have described. This fact is not only important as a complete answer to a possible objection to Young's theory, but is also, as will readily be seen, of the greatest importance for understanding the real value of our sensations of colour. In order to describe the experiment I must first give an account of a new series of phenomena.

The result of nervous action is fatigue, and this will be proportioned to the activity of the function performed, and the time of its continuance. The blood, on the other hand, which flows in through the arteries, is constantly performing its function, replacing used material by fresh, and thus carrying away the chemical results of func-

tional activity; that is to say, removing the source of fatigue.

The process of fatigue as the result of nervous action, takes place in the eye as well as other organs. When the entire retina becomes tired, as when we spend some time in the open air in brilliant sunshine, it becomes insensible to weaker light, so that if we pass immediately into a dimly lighted room we see nothing at first; we are blinded, as we call it, by the previous brightness. After a time the eye recovers itself, and at last we are able to see, and even to read, by the same dim light which at first appeared complete darkness.

It is thus that fatigue of the entire retina shows itself. But it is possible for separate parts of that membrane to become exhausted, if they alone have received a strong light. If we look steadily for some time at any bright object, surrounded by a dark background—it is necessary to look steadily in order that the image may remain quiet upon the retina, and thus fatigue a sharply defined portion of its surface—and afterwards turn our eyes upon a uniform dark-grey surface, we see projected upon it an *after-image* of the bright object we were looking at just before, with the same outline but with reversed illumination. What was dark appears bright, and what was bright dark, like the first negative of a photographer. By carefully fixing the attention, it is possible to produce very elaborate after-images, so much so that occasionally even printing can be distinguished in them. This phenomenon is the result of a local fatigue of the retina. Those parts of the membrane upon which the bright light fell before, are now less sensitive to the light of the dark-grey background than the neighbouring regions, and there now appears a dark spot upon the really uniform surface, corresponding in extent to the surface of the retina which before received the bright light.

(I may here remark that illuminated sheets of white paper are sufficiently bright to produce this after-image. If we look at much brighter objects—at flames, or at the sun itself—the effect becomes complicated. The strong excitement of the retina does not pass away immediately, but produces a direct or positive after-image, which at first unites with the negative or indirect one produced by the fatigue of the retina. Besides this, the effects of the different colours of white light differ both in duration and intensity, so that the after-images become coloured, and the whole phenomenon much more complicated.)

By means of these after-images it is easy to convince oneself that the impression produced by a bright surface begins to diminish after the first second, and that by the end of a single minute it has lost from a quarter to half of its intensity. The simplest form of experiment for this object is as follows. Cover half of a white sheet of paper with a black one, fix the eye upon some point of the white sheet near the margin of the black, and after 30 to 60 seconds draw the black sheet quickly away, without losing sight of the point. The half of the white sheet which is then exposed appears suddenly of the most brilliant brightness; and thus it becomes apparent how very much the first impression produced by the upper half of the sheet had become blunted and weakened, even in the short time taken by the experiment. And yet, what is also important to remark, the observer does not at all notice this fact, until the contrast brings it before him.

Lastly, it is possible to produce a partial fatigue of the retina in another way. We may tire it for certain colours only, by exposing either the entire retina, or a portion of it, for a certain time (from half a minute to five minutes) to one and the same colour. According to Young's theory, only one or two kinds of the optic nerve fibres will then be fatigued, those namely which are sensitive to impres-

sions of the colour in question. All the rest will remain unaffected. The result is, that when the after-image appears, red, we will suppose, upon a grey background, the uniformly mixed light of the latter can only produce sensations of green and violet in the part of the retina which has become fatigued by red light. This part is made red-blind for the time. The after-image accordingly appears of a bluish green, the complementary colour to red.

It is by this means that we are able to produce in the retina the pure and primitive sensations of saturated colours. If, for instance, we wish to see pure red, we fatigue a part of our retina by the bluish green of the spectrum, which is the complementary colour of red. We thus make this part at once green-blind and violet-blind. We then throw the after-image upon the red of as perfect a prismatic spectrum as possible; the image immediately appears in full and burning red, while the red light of the spectrum which surrounds it, although the purest that the world can offer, now seems to the unfatigued part of the retina, less saturated than the after-image, and looks as if it were covered by a whitish mist.

These facts are perhaps enough. I will not accumulate further details, to understand which it would be necessary to enter upon lengthy descriptions of many separate experiments.

We have already seen enough to answer the question whether it is possible to maintain the natural and innate conviction that the quality of our sensations, and especially our sensations of sight, give us a true impression of corresponding qualities in the outer world. It is clear that they do not. The question was really decided by Johannes Müller's deduction from well ascertained facts of the law of specific nervous energy. Whether the rays

of the sun appear to us as colour, or as warmth, does not at all depend upon their own properties, but simply upon whether they excite the fibres of the optic nerve, or those of the skin. Pressure upon the eyeball, a feeble current of electricity passed through it, a narcotic drug carried to the retina by the blood, are capable of exciting the sensation of light just as well as the sunbeams. The most complete difference offered by our several sensations, that namely between those of sight, of hearing, of taste, of smell, and of touch—this deepest of all distinctions, so deep that it is impossible to draw any comparison of likeness, or unlikeness, between the sensations of colour and of musical tones—does not, as we now see, at all depend upon the nature of the external object, but solely upon the central connections of the nerves which are affected.

We now see that the question whether within the special range of each particular sense it is possible to discover a coincidence between its objects and the sensations they produce is of only subordinate interest. What colour the waves of ether shall appear to us when they are perceived by the optic nerve depends upon their length. The system of naturally visible colours offers us a series of varieties in the composition of light, but the number of those varieties is wonderfully reduced from an unlimited number to only three. Inasmuch as the most important property of the eye is its minute appreciation of locality, and as it is so much more perfectly organised for this purpose than the ear, we may be well content that it is capable of recognising comparatively few differences in quality of light; the ear, which in the latter respect is so enormously better provided, has scarcely any power of appreciating differences of locality. But it is certainly matter for astonishment to any one who

trusts to the direct information of his natural senses, that neither the limits within which the spectrum affects our eyes, nor the differences of colour which alone remain as the simplified effect of all the actual differences of light in kind, should have any other demonstrable import than for the sense of sight. Light which is precisely the same to our eyes, may in all other physical and chemical effects be completely different. Lastly, we find that the unmixed primitive elements of all our sensations of colour (the perception of the simple primary tints) cannot be produced by any kind of external light in the natural unfatigued condition of the eye. These elementary sensations of colour can only be called forth by artificial preparation of the organ, so that, in fact, they only exist as subjective phenomena. We see, therefore, that as to any correspondence in kind of external light with the sensations it produces, there is only one bond of connection between them, a bond which at first sight may seem slender enough, but is in fact quite sufficient to lead to an infinite number of most useful applications. This law of correspondence between what is subjective and objective in vision is as follows:—

Similar light produces under like conditions a like sensation of colour. Light which under like conditions excites unlike sensations of colour is dissimilar.

When two relations correspond to one another in this manner, the one is a *sign* for the other. Hitherto the notions of a 'sign' and of an 'image' or representation have not been carefully enough distinguished in the theory of perception; and this seems to me to have been the source of numberless mistakes and false hypotheses. In an 'image' the representation must be of the same kind as that which is represented. Indeed, it is only so far an image as it is like in kind. A statue is an image of a man, so far as its form reproduces his: even if it is

executed on a smaller scale, every dimension will be represented in proportion. A picture is an image or representation of the original, first because it represents the colours of the latter by similar colours, secondly because it represents a part of its relations in space—those, namely, which belong to perspective—by corresponding relations in space.

Functional cerebral activity and the mental conceptions which go with it may be 'images' of actual occurrences in the outer world, so far as the former represent the sequence in time of the latter, so far as they represent likeness of objects by likeness of signs—that is, a regular arrangement by a regular arrangement.

This is obviously sufficient to enable the understanding to deduce what is constant from the varied changes of the external world, and to formulate it as a notion or a law. That it is also sufficient for all practical purposes we shall see in the next chapter. But not only uneducated persons, who are accustomed to trust blindly to their senses, even the educated, who know that their senses may be deceived, are inclined to demur to so complete a want of any closer correspondence in kind between actual objects and the sensations they produce than the law I have just expounded. For instance, natural philosophers long hesitated to admit the identity of the rays of light and of heat, and exhausted all possible means of escaping a conclusion which seemed to contradict the evidence of their senses.

Another example is that of Goethe, as I have endeavoured to show elsewhere. He was led to contradict Newton's theory of colours, because he could not persuade himself that white, which appears to our sensation as the purest manifestation of the brightest light, could be composed of darker colours. It was Newton's discovery of

the composition of light that was the first germ of the modern doctrine of the true functions of the senses; and in the writings of his contemporary, Locke, were correctly laid down the most important principles on which the right interpretation of sensible qualities depends. But, however clearly we may feel that here lies the difficulty for a large number of people, I have never found the opposite conviction of certainty derived from the senses so distinctly expressed that it is possible to lay hold of the point of error; and the reason seems to me to lie in the fact that beneath the popular notions on the subject lie other and more fundamentally erroneous conceptions.

We must not be led astray by confounding the notions of a *phenomenon* and an *appearance*. The colours of objects are phenomena caused by certain real differences in their constitution. They are, according to the scientific as well as to the uninstructed view, no mere appearance, even though the way in which they appear depends chiefly upon the constitution of our nervous system. A 'deceptive appearance' is the result of the normal phenomena of one object being confounded with those of another. But the sensation of colour is by no means deceptive appearance. There is no other way in which colour can appear; so that there is nothing which we could describe as the normal phenomenon, in distinction from the impressions of colour received through the eye.

Here the principal difficulty seems to me to lie in the notion of *quality*. All difficulty vanishes as soon as we clearly understand that each quality or property of a thing is, in reality, nothing else but its capability of exercising certain effects upon other things. These actions either go on between similar parts of the same body, and so produce the differences of its aggregate condition; or

they proceed from one body upon another, as in the case of chemical reactions; or they produce their effect on our organs of special sense, and are there recognised as *sensations*, as those of sight, with which we have now to do. Any of these actions is called a 'property,' when its object is understood without being expressly mentioned. Thus, when we speak of the 'solubility' of a substance, we mean its behaviour toward *water*; when we speak of its 'weight,' we mean its attraction to *the earth*; and in the same way we may correctly call a substance 'blue,' understanding, as a tacit assumption, that we are only speaking of its action upon *a normal eye*.

But if what we call a property always implies an action of one thing on another, then a property or quality can never depend upon the nature of one agent alone, but exists only in relation to, and dependent on, the nature of some second object, which is acted upon. Hence, there is really no meaning in talking of properties of light which belong to it absolutely, independent of all other objects, and which we may expect to find represented in the sensations of the human eye. The notion of such properties is a contradiction in itself. They cannot possibly exist, and therefore we cannot expect to find any coincidence of our sensations of colour with qualities of light.

These considerations have naturally long ago suggested themselves to thoughtful minds; they may be found clearly expressed in the writings of Locke and Herbart,[1] and they are completely in accordance with Kant's philosophy. But in former times, they demanded a more than usual power of abstraction, in order that their truth should be understood; whereas now the facts

[1] Johann Friedrich Herbart, born 1776, died 1841, professor of philosophy at Königsberg and Göttingen, author of *Psychologie als Wissenschaft, neugegrundet auf Erfahrung, Metaphysik und Mathematik*.—Tr.

which we have laid before the reader illustrate them in the clearest manner.

After this excursion into the world of abstract ideas, we return once more to the subject of colour, and will now examine it as a sensible 'sign' of certain external qualities, either of light itself or of the objects which reflect it.

It is essential for a good sign to be constant—that is, the same sign must always denote the same object. Now, we have already seen that in this particular our sensations of colour are imperfect; they are not quite uniform over the entire field of the retina. But the constant movement of the eye supplies this imperfection, in the same way as it makes up for the unequal sensitiveness of the different parts of the retina to form.

We have also seen that when the retina becomes tired, the intensity of the impression produced on it rapidly diminishes, but here again the usual effect of the constant movements of the eye is to equalise the fatigue of the various parts, and hence we rarely see after-images. If they appear at all, it is in the case of brilliant objects like very bright flames, or the sun itself. And, so long as the fatigue of the entire retina is uniform, the relative brightness and colour of the different objects in sight remains almost unchanged, so that the effect of fatigue is gradually to weaken the apparent illumination of the entire field of vision.

This brings us to consider the differences in the pictures presented by the eye, which depend on different degrees of illumination. Here again we meet with instructive facts. We look at external objects under light of very different intensity, varying from the most dazzling sunshine to the pale beams of the moon; and the light of the full moon is 150,000 times less than that of the sun.

Moreover, the colour of the illumination may vary greatly. Thus, we sometimes employ artificial light, and this is always more or less orange in colour; or the natural daylight is altered, as we see it in the green shade of an arbour, or in a room with coloured carpets and curtains. As the brightness and the colour of the illumination changes, so of course will the brightness and colour of the light which the illuminated objects reflect to our eyes, since all differences in local colour depend upon different bodies reflecting and absorbing various proportions of the several rays of the sun. Cinnabar reflects the rays of great wave length without any obvious loss, while it absorbs almost the whole of the other rays. Accordingly, this substance appears of the same red colour as the beams which it throws back into the eye. If it is illuminated with light of some other colour, without any mixture of red, it appears almost black.

These observations teach what we find confirmed by daily experience in a hundred ways, that the apparent colour and brightness of illuminated objects varies with the colour and brightness of the illumination. This is a fact of the first importance for the painter, for many of his finest effects depend on it.

But what is most important practically is for us to be able to recognise surrounding objects when we see them: it is only seldom that, for some artistic or scientific purpose, we turn our attention to the way in which they are illuminated. Now what is constant in the colour of an object is not the brightness and colour of the light which it reflects, but the relation between the intensity of the different coloured constituents of this light, on the one hand, and that of the corresponding constituents of the light which illuminates it on the other. This proportion alone is the expression of a constant property of the object in question.

Considered theoretically, the task of judging of the colour of a body under changing illumination would seem to be impossible; but in practice we soon find that we are able to judge of local colour without the least uncertainty or hesitation, and under the most different conditions. For instance, white paper in full moonlight is darker than black satin in daylight, but we never find any difficulty in recognising the paper as white and the satin as black. Indeed, it is much more difficult to satisfy ourselves that a dark object with the sun shining on it reflects light of exactly the same colour, and perhaps the same brightness, as a white object in shadow, than that the proper colour of a white paper in shadow is the same as that of a sheet of the same kind lying close to it in the sunlight. Grey seems to us something altogether different from white, and so it is, regarded as a *proper* colour;[1] for anything which only reflects half the light it receives must have a different surface from one which reflects it all. And yet the impression upon the retina of a grey surface under illumination may be absolutely identical with that of a white surface in the shade. Every painter represents a white object in shadow by means of grey pigment, and if he has correctly imitated nature, it appears pure white. In order to convince one's self of the identity in this respect —i.e. as *illumination* colours—of grey and white, the following experiment may be tried. Cut out a circle in grey paper, and concentrate a strong beam of light upon it with a lens, so that the limits of the illumination exactly correspond with those of the grey circle. It will

[1] The local or proper colour of an object (*Körperfarbe*) is that which it shows in common white light, while the 'illumination-colour,' as I have translated *Lichtfarbe*, is that which is produced by coloured light. Thus the red of some sandstone rocks seen by common white light is their proper colour, that of a snow mountain in the rays of the setting sun is an illumination-colour.—Tr.

then be impossible to tell that there is any artificial illumination at all. The grey looks white.[1]

We may assume, and the assumption is justified by certain phenomena of contrast, that illumination of the brightest white we can produce, gives a true criterion for judging of the darker objects in the neighbourhood, since, under ordinary circumstances, the brightness of any proper colour diminishes in proportion as the illumination is diminished, or the fatigue of the retina increased.

This relation holds even for extreme degrees of illumination, so far as the objective intensity of the light is concerned, but not for our sensation. Under illumination so brilliant as to approach what would be blinding, degrees of brightness of light-coloured objects become less and less distinguishable; and, in the same way, when the illumination is very feeble, we are unable to appreciate slight differences in the amount of light reflected by dark objects. The result is that in sunshine local colours of moderate brightness approach the brightest, whereas in moonlight they approach the darkest. The painter utilises this difference in order to represent noonday or midnight scenes, although pictures, which are usually seen in uniform daylight, do not really admit of any difference of brightness approaching that between sunshine and moonlight. To represent the former, he paints the objects of moderate brightness almost as bright as the brightest; for the latter, he makes them almost as dark as the darkest.

The effect is assisted by another difference in the sensation produced by the same actual conditions of light and colour. If the brightness of various colours is equally increased, that of red and yellow becomes apparently stronger than that of blue. Thus, if we select a red and a blue paper which appear of the same brightness in ordinary daylight,

[1] The demonstration is more striking if the grey disk is placed on a sheet of white paper in diffused light.—Tr.

the red seems much brighter in full sunlight, the blue in moonlight or starlight. This peculiarity in our perception is also made use of by painters; they make yellow tints predominate when representing landscapes in full sunshine, while every object of a moonlight scene is given a shade of blue. But it is not only local colour which is thus affected; the same is true of the colours of the spectrum.

These examples show very plainly how independent our judgment of colours is of their actual amount of illumination. In the same way, it is scarcely affected by the colour of the illumination. We know, of course, in a general way that candle-light is yellowish compared with daylight, but we only learn to appreciate how much the two kinds of illumination differ in colour when we bring them together of the same intensity—as, for example, in the experiment of *coloured shadows*. If we admit light from a cloudy sky through a narrow opening into a dark room, so that it falls sideways on a horizontal sheet of white paper, while candle-light falls on it from the other side, and if we then hold a pencil vertically upon the paper, it will of course throw two shadows: the one made by the daylight will be orange, and looks so; the other made by the candle-light is really white, but appears blue by contrast. The blue and the orange of the two shadows are both colours which we call white, when we see them by daylight and candle-light respectively. Seen together, they appear as two very different and tolerably saturated colours, yet we do not hesitate a moment in recognising white paper by candle-light as white, and very different from orange.[1]

The most remarkable of this series of facts is that we can separate the colour of any transparent medium from

[1] This experiment with diffused white day-light may also be made with moonlight.

that of objects seen through it. This is proved by a number of experiments contrived to illustrate the effects of contrast. If we look through a green veil at a field of snow, although the light reflected from it must really have a greenish tint when it reaches our eyes, yet it appears, on the contrary, of a reddish tint, from the effect of the indirect after-image of green. So completely are we able to separate the light which belongs to the transparent medium from that of the objects seen through it.[1]

The changes of colour in the two last experiments are known as phenomena of contrast. They consist in mistakes as to local colour, which for the most part depend upon imperfectly defined after-images.[2] This effect is known as *successive contrast*, and is experienced when the eye passes over a series of coloured objects. But a similar mistake may result from our custom of judging of local colour according to the brightness and colour of the various objects seen at the same time. If these relations happen to be different from what is usual, contrast phenomena ensue. When, for example, objects are seen under two different coloured illuminations, or through two different coloured media (whether real or apparent), these conditions produce what is called *simultaneous contrast*. Thus in the experiment described above of coloured shadows thrown by daylight and candle-light, the doubly illuminated surface of the paper being the brightest object seen, gives a false criterion for white. Compared with it, the really white but less bright light of the shadow thrown by the candle looks blue. Moreover, in these curious effects of contrast, we must take into account

[1] A number of similar experiments will be found described in the author's *Handbuch der physiologischen Optik*, pp. 398–411.

[2] These after-images have been described as 'accidental images,' positive when of the same colour as the original colour, negative when of the complementary colour.—Tr.

that differences in sensation which are easily apprehended appear to us greater than those less obvious. Differences of colour which are actually before our eyes are more easily apprehended than those which we only keep in memory, and contrasts between objects which are close to one another in the field of vision are more easily recognised than when they are at a distance. All this contributes to the effect. Indeed, there are a number of subordinate circumstances affecting the result which it would be very interesting to follow out in detail, for they throw great light upon the way in which we judge of local colour: but we must not pursue the inquiry further here. I will only remark that all these effects of contrast are not less interesting for the scientific painter than for the physiologist, since he must often exaggerate the natural phenomena of contrast, in order to produce the impression of greater varieties of light and greater fulness of colour than can be actually produced by artificial pigments.

Here we must leave the theory of the Sensations of Sight. This part of our inquiry has shown us that the qualities of these sensations can only be regarded as *signs* of certain different qualities, which belong sometimes to light itself, sometimes to the bodies it illuminates, but that there is not a single actual quality of the objects seen which precisely corresponds to our sensations of sight. Nay, we have seen that, even regarded as signs of real phenomena in the outer world, they do not possess the one essential requisite of a complete system of signs—namely, constancy—with anything like completeness; so that all that we can say of our sensations of sight is, that 'under similar conditions, the qualities of this sensation appear in the same way for the same objects.'

And yet, in spite of all this imperfection, we have also

found that by means of so inconstant a system of signs, we are able to accomplish the most important part of our task—to recognise the same proper colours wherever they occur; and, considering the difficulties in the way, it is surprising how well we succeed. Out of this inconstant system of brightness and of colours, varying according to the illumination, varying according to the fatigue of the retina, varying according to the part of it affected, we are able to determine the proper colour of any object, the one constant phenomenon which corresponds to a constant quality of its surface; and this we can do, not after long consideration, but by an instantaneous and involuntary decision.

The inaccuracies and imperfections of the eye as an optical instrument, and those which belong to the image on the retina, now appear insignificant in comparison with the incongruities which we have met with in the field of sensation. One might almost believe that Nature had here contradicted herself on purpose, in order to destroy any dream of a pre-existing harmony between the outer and the inner world.

And what progress have we made in our task of explaining Sight? It might seem that we are farther off than ever; the riddle only more complicated, and less hope than ever of finding out the answer. The reader may perhaps feel inclined to reproach Science with only knowing how to break up with fruitless criticism the fair world presented to us by our senses, in order to annihilate the fragments.

> Woe! woe!
> Thou hast destroyed
> The beautiful world
> With powerful fist;
> In ruin 'tis hurled,
> By the blow of a demigod shattered.

The scattered
Fragments into the void we carry,
Deploring
The beauty perished beyond restoring.[1]

and may feel determined to stick fast to the 'sound common sense' of mankind, and believe his own senses more than physiology.

But there is still a part of our investigation which we have not touched—that into our conceptions of space. Let us see whether, after all, our natural reliance upon the accuracy of what our senses teach us, will not be justified even before the tribunal of Science.

III. The Perception of Sight.

The colours which have been the subject of the last chapter are not only an ornament we should be sorry to lose, but are also a means of assisting us in the distinction and recognition of external objects. But the importance of *colour* for this purpose is far less than the means which the rapid and far-reaching power of the eye gives us of distinguishing the various relations of *locality*. No other sense can be compared with the eye in this respect. The sense of touch, it is true, can distinguish relations of space, and has the special power of judging of all matter

[1] Bayard Taylor's translation of the passage in *Faust*:—

Du hast sie zerstört
Die schöne Welt
Mit mächtiger Faust;
Sie stürzt, sie zerfällt,
Ein Halbgott hat sie zerschlagen.
Wir tragen
Die Trümmern ins Nichts hinüber,
Und klagen
Ueber die verlorne Schöne.

thin reach, at once as to resistance, volume, and weight: but the range of touch is limited, and the distinction it can make between small distances is not nearly so accurate as that of sight. Yet the sense of touch is sufficient, as experiments upon persons born blind have proved, to develop complete notions of space. This proves that the possession of sight is not necessary for the formation of these conceptions, and we shall soon see that we are continually controlling and correcting the notions of locality derived from the eye by the help of the sense of touch, and always accept the impressions on the latter sense as decisive. The two senses, which really have the same task, though with very different means of accomplishing it, happily supply each other's deficiencies. Touch is a trustworthy and experienced servant, but enjoys only a limited range, while sight rivals the boldest flights of fancy in penetrating to illimitable distances.

This combination of the two senses is of great importance for our present task; for, since we have here only to do with vision, and since touch is sufficient to produce complete conceptions of locality, we may assume these conceptions to be already complete, at least in their general outline, and may, accordingly, confine our investigation to ascertaining the common point of agreement between the visual and tactile perceptions of space. The question how it is possible for any conception of locality to arise from either or both of these sensations, we will leave till last.

It is obvious, from a consideration of well-known facts, that the distribution of our sensations among nervous structures separated from one another does not at all necessarily bring with it the conception that the causes of these sensations are locally separate. For example, we may have sensations of light, of warmth, of various notes of music, and also perhaps of an odour, in the same room,

and may recognise that all these agents are diffused through the air of the room at the same time, and without any difference of locality. When a compound colour falls upon the retina, we are conscious of three separate elementary impressions, probably conveyed by separate nerves, without any power of distinguishing them. We hear in a note struck on a stringed instrument or in the human voice, different tones at the same time, one fundamental, and a series of harmonic overtones, which also are probably received by different nerves, and yet we are unable to separate them in space. Many articles of food produce a different impression of taste upon different parts of the tongue, and also produce sensations of odour by their volatile particles ascending into the nostrils from behind. But these different sensations, recognised by different parts of the nervous system, are usually completely and inseparably united in the compound sen sation which we call taste.

No doubt, with a little attention it is possible to ascertain the parts of the body which receive these sensations, but, even when these are known to be locally separate, it does not follow that we must conceive of the sources of these sensations as separated in the same way.

We find a corresponding fact in the physiology of sight —namely, that we see only a single object with our two eyes, although the impression is conveyed by two distinct nerves. In fact, both phenomena are examples of a more universal law.

Hence, when we find that a plane optical image of the objects in the field of vision is produced on the retina, and that the different parts of this image excite different fibres of the optic nerve, this is not a sufficient ground for our referring the sensations thus produced to locally distinct regions of our field of vision. Something else

must clearly be added to produce the notion of separation in space.

The sense of touch offers precisely the same problem. When two different parts of the skin are touched at the same time, two different sensitive nerves are excited, but the local separation between these two nerves is not a sufficient ground for our recognition of the two parts which have been touched as distinct, and for the conception of two different external objects which follows. Indeed, this conception will vary according to circumstances. If we touch the table with two fingers, and feel under each a grain of sand, we suppose that there are two separate grains of sand; but if we place the two fingers one against the other, and a grain of sand between them, we may have the same sensations of touch in the same two nerves as before, and yet, under these circumstances, we suppose that there is only a single grain. In this case, our consciousness of the position of the fingers has obviously an influence upon the result at which the mind arrives. This is further proved by the experiment of crossing two fingers one over the other, and putting a marble between them, when the single object will produce in the mind the conception of two.

What, then, is it which comes to help the anatomical distinction in locality between the different sensitive nerves, and, in cases like those I have mentioned, produces the notion of separation in space? In attempting to answer this question, we cannot avoid a controversy which has not yet been decided.

Some physiologists, following the lead of Johannes Müller, would answer that the retina or skin, being itself an organ which is extended in space, perceives impressions which carry with them this quality of extension in space;

difference. If it were not so, it would be impossible to distinguish any local difference in the field of vision. The sensation of red, when it falls upon the right side of the retina, must in some way be different from the sensation of the same red when it affects the left side; and, moreover, *this* difference between the two sensations must be of another kind from that which we recognise when the same spot in the retina is successively affected by two different shades of red. Lotze[1] has named this difference between the sensations which the same colour excites when it affects different parts of the retina, the *local sign* of the sensation. We are for the present ignorant of the nature of this difference, but I adopt the name given by Lotze as a convenient expression. While it would be premature to form any further hypothesis as to the nature of these 'local signs,' there can be no doubt of their existence, for it follows from the fact that we are able to distinguish local differences in the field of vision.

The difference, therefore, between the two opposing views is as follows. The Empirical Theory regards the local signs (whatever they really may be) as signs the signification of which must be learnt, and is actually learnt, in order to arrive at a knowledge of the external world. It is not at all necessary to suppose any kind of correspondence between these local signs and the actual differences of locality which they signify. The Innate Theory, on the other hand, supposes that the local signs are nothing else than direct conceptions of differences in space as such, both in their nature and their magnitude.

[1] Rudolf Hermann Lotze, Professor in the University of Göttingen, originally a disciple of Herbart (v. *supra*), author of *Allgemeine Physiologie des menschlichen Körpers*, 1851.—Tr.

The reader will see how the subject of our present enquiry involves the consideration of that far-reaching opposition between the system of philosophy which assumes a pre-existing harmony of the laws of mental operations with those of the outer world, and the system which attempts to derive all correspondence between mind and matter from the results of experience.

So long as we confine ourselves to the observation of a field of two dimensions, the individual parts of which offer no, or, at any rate, no recognisable, difference in their distances from the eye—so long, for instance, as we only look at the sky and distant parts of the landscape, both the above theories practically offer an equally good explanation of the way in which we form conceptions of local relations in the field of vision. The extension of the retinal image corresponds to the extension of the actual image presented by the objects before us; or, at all events, there are no incongruities which may not be reconciled with the Innate Theory of sight without any very difficult assumptions or explanations.

The first of these incongruities is that in the retinal picture the top and bottom and the right and left of the actual image are inverted. This is seen in Fig. 30 to result from the rays of light crossing as they enter the pupil; the point a is the retinal image of A, b of B. This has always been a difficulty in the theory of vision, and many hypotheses have been invented to explain it. Two of these have survived. We may, with Johannes Müller, regard the conception of upper and lower as only a relative distinction, so far as sight is concerned—that is, as only affecting the relation of the one to the other; and we must further suppose that the feeling of correspondence between what is upper in the sense of sight and in the sense of touch is only acquired by experience,

when we see the hands, which feel, moving in the field of vision. Or, secondly, we may assume with Fick[1] that, since all impressions upon the retina must be conveyed to the brain in order to be there perceived, the nerves of sight and those of feeling are so arranged in the brain as to produce a correspondence between the notions they suggest of upper and under, right and left. This supposition has, however, no pretence of any anatomical facts to support it.

The second difficulty for the Intuitive Theory is that, while we have two retinal pictures, we do not see double. This difficulty was met by the assumption that both retinæ when they are excited produce only a single sensation in the brain, and that the several points of each retina correspond with each other, so that each pair of corresponding or 'identical' points produces the sensation of a single one. Now there is an actual anatomical arrangement which might perhaps support this hypothesis. The two optic nerves cross before entering the brain, and thus become united. Pathological observations make it probable that the nerve-fibres from the right-hand halves of both retinæ pass to the right cerebral hemisphere, those from the left halves to the left hemisphere.[2] But although corresponding nerve-fibres would thus be brought close together, it has not yet been shown that they actually unite in the brain.

[1] Ludwig Fick, late Professor of Medicine in the University of Marburg, the brother of Prof. Adolf Fick, of Zürich.

[2] We may compare the arrangement to that of the reins of a pair of horses: the inner fibres only of each optic nerve cross, so that those which run to the right half of the brain are the outer fibres of the right and the inner of the left retina, while those which run to the left cerebral hemisphere are the outer fibres of the left and the inner of the right retina: just as the inner reins of both horses cross, so that the outer rein of the off horse and the inner of the near one run together to the driver's right hand, while the inner rein of the off and the outer of the near horse pass to his left hand.—Tr.

These two difficulties do not apply to the Empirical Theory, since it only supposes that the actual sensible 'sign,' whether it be simple or complex, is recognised as the sign of that which it signifies. An uninstructed person is as sure as possible of the notions he derives from his eyesight, without ever knowing that he has two retinæ, that there is an inverted picture on each, or that there is such a thing as an optic nerve to be excited, or a brain to receive the impression. He is not troubled by his retinal images being inverted and double. He knows what impression such and such an object in such and such a position makes on him through his eyesight, and governs himself accordingly. But the possibility of learning the signification of the local signs which belong to our sensations of sight, so as to be able to recognise the actual relations which they denote, depends, first, on our having movable parts of our own body within sight; so that, when we once know by means of touch what relation in space and what movement is, we can further learn what changes in the impressions on the eye correspond to the voluntary movements of a hand which we can see. In the second place, when we move our eyes while looking at a field of vision filled with objects at rest, the retina, as it moves, changes its relation to the almost unchanged position of the retinal picture. We thus learn what impression the same object makes upon different parts of the retina. An unchanged retinal picture, passing over the retina as the eye turns, is like a pair of compasses which we move over a drawing in order to measure its parts. Even if the 'local signs' of sensation were quite arbitrary, thrown together without any systematic arrangement (a supposition which I regard as improbable), it would still be possible by means of the movements of the hand and of the eye, as just described,

to ascertain which signs go together, and which correspond in different regions of the retina to points at similar distances in the two dimensions of the field of vision. This is in accordance with experiments by Fechner,[1] Volkmann,[2] and myself, which prove that even the fully developed eye of an adult can only accurately compare the size of those lines or angles in the field of vision, the images of which can be thrown one after another upon precisely the same spot of the retina by means of the ordinary movements of the eye.

Moreover, we may convince ourselves by a simple experiment that the harmonious results of the perceptions of feeling and of sight depend, even in the adult, upon a constant comparison of the two, by means of the retinal pictures of our hands as they move. If we put on a pair of spectacles with prismatic glasses, the two flat surfaces of which converge towards the right, all objects appear to be moved over to the right. If we now try to touch anything we see, taking care to shut the eyes before the hand appears in sight, it passes to the right of the object; but if we follow the movement of the hand with the eye, we are able to touch what we intend, by bringing the retinal image of the hand up to that of the object. Again, if we handle the object for one or two minutes, watching it all the time, a fresh correspondence is formed between the eye and the hand, in spite of the deceptive glass, so that we are now able to touch the object with perfect certainty, even when the eyes are shut. And we can even do the same with the other hand without seeing it, which proves that it is not the perception of touch

[1] Gustav Theodor Fechner, author of *Elemente der Psychophysik*, 1860; also known as a satirist.—Tr.

[2] Alfred Wilhelm Volkmann, successively Professor of Physiology at Leipzig, Dorpat, and Halle; author of *Physiologische Untersuchungen im Gebiete der Optik*, 1864, &c.—Tr.

which has been rectified by comparison with the false retinal images, but, on the contrary, the perception of sight which has been corrected by that of touch. But, again, if, after trying this experiment several times, we take off the spectacles and then look at any object, taking care not to bring our hands into the field of vision, and now try to touch it with our eyes shut, the hand will pass beyond it on the opposite side—that is, to the left. The new harmony which was established between the perceptions of sight and of touch continues its effects, and thus leads to fresh mistakes when the normal conditions are restored.

In preparing objects with needles under a compound microscope, we must learn to harmonise the inverted microscopical image with our muscular sense; and we have to get over a similar difficulty in shaving before a looking-glass, which changes right to left.

These instances, in which the image presented in the two dimensions of the field of vision is essentially of the same kind as the retinal images, and resembles them, can be equally well explained (or nearly so) by the two opposite theories of vision to which I have referred. But it is quite another matter when we pass to the observation of near objects of three dimensions. In this case there is a thorough and complete incongruity between our retinal images on the one hand, and, on the other, the actual condition of the objects as well as the correct impression of them which we receive. Here we are compelled to choose between the two opposite theories, and accordingly this department of our subject—the explanation of our Perception of Solidity or *Depth* in the field of vision, and that of binocular vision on which the former chiefly depends—has for many years become the field of much investigation and no little controversy. And no won-

der, for we have already learned enough to see that the questions which have here to be decided are of fundamental importance, not only for the physiology of sight, but for a correct understanding of the true nature and limits of human knowledge generally.

Each of our eyes projects a plane image upon its own retina. However we may suppose the conducting nerves to be arranged, the two retinal images when united in the brain can only reappear as a plane image. But instead of the two plane retinal images, we find that the actual impression on our mind is a solid image of three dimensions. Here, again, as in the system of colours, the outer world is richer than our sensation by one dimension; but in this case the conception formed by the mind completely represents the reality of the outer world. It is important to remember that this perception of depth is fully as vivid, direct, and exact as that of the plane dimensions of the field of vision. If a man takes a leap from one rock to another, his life depends just as much upon his rightly estimating the distance of the rock on which he is to alight, as upon his not misjudging its position, right or left; and, as a matter of experience, we find that we can do the one just as quickly and as surely as the other.

In what way can this appreciation of what we call depth, solidity, and direct distance come about? Let us first ascertain what are the facts.

At the outset of the enquiry we must bear in mind that the perception of the solid form of objects and of their relative distance from us is not quite absent, even when we look at them with only one eye and without changing our position. Now the means which we possess in this case are just the same as those which the painter can employ in order to give the figures on his canvas the appearance of being solid objects, and of

standing at different distances from the spectator. It is part of a painter's merit for his figures to stand out boldly. Now how does he produce the illusion? We shall find, in the first place, that in painting a landscape he likes to have the sun near the horizon, which gives him strong shadows; for these throw objects in the foreground into bold relief. Next he prefers an atmosphere which is not quite clear, because slight obscurity makes the distance appear far off. Then he is fond of bringing in figures of men and cattle, because, by help of these objects of known size, we can easily measure the size and distance of other parts of the scene. Lastly, houses and other regular productions of art are also useful for giving a clue to the meaning of the picture, since they enable us easily to recognise the position of horizontal surfaces. The representation of solid forms by drawings in correct perspective is most successful in the case of objects of regular and symmetrical shape, such as buildings, machines, and implements of various kinds. For we know that all of these are chiefly bounded either by planes which meet at a right angle or by spherical and cylindrical surfaces; and this is sufficient to supply what the drawing does not directly show. Moreover, in the case of figures of men or animals, our knowledge that the two sides are symmetrical further assists the impression conveyed.

But objects of unknown and irregular shape, as rocks or masses of ice, baffle the skill of the most consummate artist; and even their representation in the most complete and perfect manner possible, by means of photography, often shows nothing but a confused mass of black and white. Yet, when we have these objects in reality before our eyes, a single glance is enough for us to recognise their form.

The first who clearly showed in what points it is

Impossible for any picture to represent actual objects was the great master of painting, Leonardo da Vinci,[1] who was almost as distinguished in natural philosophy as in art. He pointed out in his *Trattato della Pittura*, that the views of the outer world presented by each of our eyes are not precisely the same. Each eye sees in its retinal image a perspective view of the objects which lie before it; but, inasmuch as it occupies a somewhat different position in space from the other, its point of view and so its whole perspective image is different. If I hold up my finger and look at it first with the right and then with the left eye, it covers, in the picture seen by the latter, a part of the opposite wall of the room which is more to the right than in the picture seen by the right eye. If I hold up my right hand with the thumb towards me, I see with the right eye more of the back of the hand, with the left more of the palm; and the same effect is produced whenever we look at bodies of which the several parts are at different distances from our eyes. But when I look at a hand represented in the same position in a painting, the right eye will see exactly the same figure as the left, and just as much of either the palm or the back of it. Thus we see that actual solid objects present different pictures to the two eyes, while a painting shows only the same. Hence follows a difference in the impression made upon the sight which the utmost perfection in a representation on a flat surface cannot supply.

The clearest proof that seeing with two eyes, and the difference of the pictures presented by each, constitute

[1] Born at Vinci, near Florence, 1452; died at Cloux, near Amboise, 1519. Mr. Hallam says of his scientific writings, that they are 'more like revelations of physical truths vouchsafed to a single mind, than the superstructure of its reasoning upon any established basis. . . . He first laid down the grand principle of Bacon, that experiment and observation must be the guides to just theory in the investigation of nature.'—Tr.

the most important cause of our perception of a third dimension in the field of vision, has been furnished by Wheatstone's invention of the stereoscope.[1] I may assume that this instrument and the peculiar illusion which it produces are well known. By its help we see the solid shape of the objects represented on the stereoscopic slide, with the same complete evidence of the senses with which we should look at the real objects themselves. This illusion is produced by presenting somewhat different pictures to the two eyes—to the right, one which represents the object in perspective as it would appear to that eye, and to the left one as it would appear to the left. If the pictures are otherwise exact and drawn from two different points of view corresponding to the position of the two eyes, as can be easily done by photography, we receive on looking into the stereoscope precisely the same impression in black and white as the object itself would give.

Anyone who has sufficient control over the movements of his eyes does not need the help of an instrument in order to combine the two pictures on a stereoscopic slide into a single solid image. It is only necessary so to direct the eyes, that each of them shall at the same time see corresponding points in the two pictures; but it is easier to do so by help of an instrument which will apparently bring the two pictures to the same place.

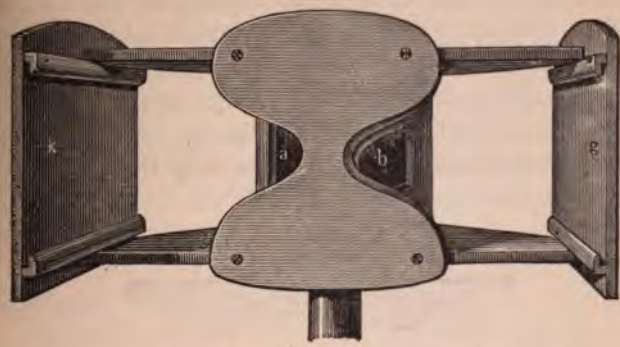
In Wheatstone's original stereoscope, represented in Fig. 35, the observer looked with the right eye into the mirror *b*, and with the left into the mirror *a*. Both mirrors were placed at an angle to the observer's line of sight, and the two pictures were so placed at *k* and *g* that their reflected images appeared at the same place behind the two mirrors; but the right eye saw the

[1] Described in the *Philosophical Transactions* for 1838.—Tr.

ïcture g in the mirror b, while the left saw the picture in the mirror a.

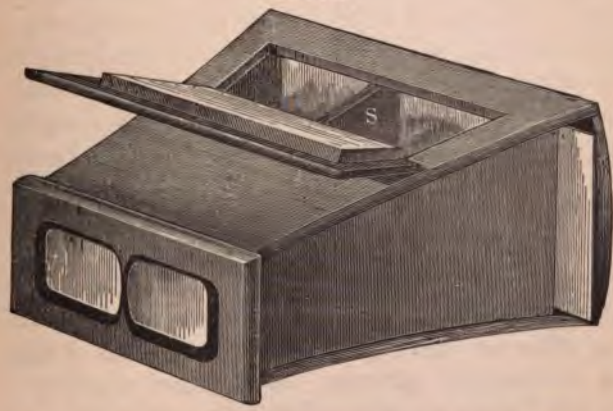
A more convenient instrument, though it does not

Fig. 35.



give such sharply defined effects, is the ordinary stereoscope of Brewster,[1] shown in Fig. 36. Here the two

Fig. 36.



pictures are placed on the same slide and laid in the

[1] Sir David Brewster, Professor of Mathematics at Edinburgh, born 1781, died 1868.—Tr.

lower part of the stereoscope, which is divided by a partition s. Two slightly prismatic glasses with convex surfaces are fixed at the top of the instrument which show the pictures somewhat further off, somewhat magnified, and at the same time overlapping each other, so that both appear to be in the middle of the instrument. The section of the double eye-piece shown in Fig. 37 exhibits the position and shape of the right and left prisms. Thus both pictures are apparently brought to the same spot, and each eye sees only the one which belongs to it.

FIG. 37.

The illusion produced by the stereoscope is most obvious and striking when other means of recognising the form of an object fail. This is the case with geometrical outlines of solid figures, such as diagrams of crystals, and also with representations of irregular objects, especially when they are transparent, so that the shadows do not fall as we are accustomed to see them in opaque objects. Thus glaciers in stereoscopic photographs often appear to the unassisted eye an incomprehensible chaos of black and white, but when seen through a stereoscope the clear transparent ice, with its fissures and polished surfaces, comes out as if it were real. It has often happened that when I have seen for the first time buildings, cities or landscapes, with which I was familiar from stereoscopic pictures, they seemed familiar to me; but I never experienced this impression after seeing any number of ordinary pictures, because these but so imperfectly represent the real effect upon the senses.

The accuracy of the stereoscope is no less wonderful. Dove[1] has contrived an ingenious illustration of this. Take two pieces of paper printed with the same type, or from the same copper-plate, and hence exactly alike, and put them in the stereoscope in place of the two ordinary photographs. They will then unite into a single completely flat image, because, as we have seen above, the two retinal images of a flat picture are identical. But no human skill is able to copy the letters of one copperplate on to another so perfectly that there shall not be some difference between them. If, therefore, we print off the same sentence from the original plate and a copy of it, or the same letters with different specimens of the same type, and put the two pieces of paper into the stereoscope, some lines will appear nearer and some further off than the rest. This is the easiest way of detecting spurious bank notes. A suspected one is put in a stereoscope along with a genuine specimen of the same kind, and it is then at once seen whether all the marks in the combined image appear on the same plane. This experiment is also important for the theory of vision, since it teaches us in a most striking manner how vivid, sure, and minute is our judgment as to depth derived from the combination of the two retinal images.

We now come to the question how is it possible for two different flat perspective images upon the retina, each of them representing only two dimensions, to combine so as to present a solid image of three dimensions.

[1] Heinrich Wilhelm Dove, Professor in the University of Berlin, author of *Optische Studien* (1859); also eminent for his researches in meteorology and electricity.

His paper, *Anwendung des Stereoskops um falsches von echtem Papiergeld zu unterscheiden*, was published in 1859.—Tr.

We must first make sure that we are really able to distinguish between the two flat images offered us by our eyes. If I hold my finger up and look towards the opposite wall, it covers a different part of the wall to each eye, as I mentioned above. Accordingly I see the finger twice, in front of two different places on the wall: and if I see a single image of the wall I must see a double image of the finger.

Now in ordinary vision we try to recognise the solid form of surrounding objects, and either do not notice this double image at all, or only when it is unusually striking. In order to see it we must look at the field of vision in another way—in the way that an artist does who intends to draw it. He tries to forget the actual shape, size, and distance of the objects that he represents. One would think that this is the more simple and original way of seeing things; and hitherto most physiologists have regarded it as the kind of vision which results most directly from sensation, while they have looked on ordinary solid vision as a secondary way of seeing things, which has to be learned as the result of experience. But every draughtsman knows how much harder it is to appreciate the apparent form in which objects appear in the field of vision, and to measure the angular distance between them, than to recognise what is their actual form and comparative size. In fact, the knowledge of the true relations of surrounding objects of which the artist cannot divest himself, is his greatest difficulty in drawing from nature.

Accordingly, if we look at the field of vision with both eyes, in the way an artist does, fixing our attention upon the outlines, as they would appear if projected on a pane of glass between us and them, we then become at once aware of the difference between the two retinal images. We see those objects double which lie further off or

nearer than the point at which we are looking, and are not too far removed from it laterally to admit of their position being sufficiently seen. At first we can only recognise double images of objects at very different distances from the eye, but by practice they will be seen with objects at nearly the same distance.

All these phenomena, and others like them, of double images of objects seen with both eyes, may be reduced to a simple rule which was laid down by Johannes Müller:—'For each point of one retina there is on the other a *corresponding* point.' In the ordinary flat field of vision presented by the two eyes, the images received by corresponding points as a rule coincide, while images received by those which do not correspond do not coincide. The corresponding points in each retina (without noticing slight deviations) are those which are situated at the same lateral and vertical distance from the point of the retina at which rays of light come to a focus when we fix the eye for exact vision, namely, the yellow spot.

The reader will remember that the intuitive theory of vision of necessity assumes a complete combination of those sensations which are excited by impressions upon *corresponding*, or, as Müller calls them, 'identical' points. This supposition was most fully expressed in the anatomical hypothesis, that two nerve fibres which arise from corresponding points of the two retinæ actually unite so as to form a single fibre, either at the commissure of the optic nerves or in the brain itself. I may, however, remark that Johannes Müller did not definitely commit himself to this mechanical explanation, although he suggested its possibility. He wished his law of identical points to be regarded simply as an expression of facts, and only insisted that the position in the field of vision of the images they receive is always the same.

But a difficulty arose. The distinction between the

double images is comparatively imperfect, whenever it is possible to combine them into a single view; a striking contrast to the extraordinary precision with which, as Dove has shown, we can judge of stereoscopic relief. Yet the latter power depends upon the same differences between the two retinal pictures which cause the phenomenon of double images. The slight difference of distance between the objects represented in the right and left half of a stereoscopic photograph, which suffices to produce the most striking effect of solidity, must be increased twenty or thirty-fold before it can be recognised in the production of a double image, even if we suppose the most careful observation by one who is well practised in the experiment.

Again, there are a number of other circumstances which make the recognition of double images either easy or difficult. The most striking instance of the latter is the effect of relief. The more vivid the impression of solidity, the more difficult are double images to see, so that it is easier to see them in stereoscopic pictures than in the actual objects they represent. On the other hand, the observation of double images is facilitated by varying the colour and brightness of the lines in the two stereoscopic pictures, or by putting lines in both which exactly correspond, and so will make more evident by contrast the imperfect coalescence of the other lines. All these circumstances ought to have no influence, if the combination of the two images in our sensation depended upon any anatomical arrangement of the conducting nerves.

Again, after the invention of the stereoscope, a fresh difficulty arose in explaining our perceptions of solidity by the differences between the two retinal images. First, Brücke[1] called attention to a series

[1] Professor of Physiology in the University of Vienna.

of facts which apparently made it possible to reconcile the new phenomena discovered with the theory of the innate identity of the sensations conveyed by the two retinæ. If we carefully follow the way in which we look at stereoscopic pictures or at real objects, we notice that the eye follows the different outlines one after another, so that we see the 'fixed point' at each moment single, while the other points appear double. But, usually, our attention is concentrated upon the fixed point, and we observe the double images so little that to many people they are a new and surprising phenomenon when first pointed out. Now since in following the outlines of these pictures, or of an actual image, we move the eyes unequally this way and that, sometimes they converge, and sometimes diverge, according as we look at points of the outline which are apparently nearer or further off; and these differences in movement may give rise to the impression of different degrees of distance of the several lines.

Now it is quite true, that by this movement of the eye while looking at stereoscopic outlines, we gain a much more clear and exact image of the raised surface they represent, than if we fix our attention upon a single point. Perhaps the simple reason is that when we move the eyes we look at every point of the figure in succession *directly*, and therefore see it much more sharply defined than when we see only one point directly and the others indirectly. But Brücke's hypothesis, that the perception of solidity is only produced by this movement of the eyes, was disproved by experiments made by Dove, which showed that the peculiar illusion of stereoscopic pictures is also produced when they are illuminated with an electric spark. The light then lasts for less than the four thousandth part of a second. In this time heavy bodies move so little, even at great velocities,

that they seem to be at rest. Hence there cannot be the slightest movement of the eye, while the spark lasts, which can possibly be recognised; and yet we receive the complete impression of stereoscopic relief.

Secondly, such a combination of the sensations of the two eyes as the anatomical hypothesis assumes, is proved not to exist by the phenomenon of *stereoscopic lustre*, which was also discovered by Dove. If the same surface is made white in one stereoscopic picture and black in another, the combined image appears to shine, though the paper itself is quite dull. Stereoscopic drawings of crystals are made so that one shows white lines on a black ground, and the other black lines on a white ground. When looked at through a stereoscope they give the impression of a solid crystal of shining graphite. By the same means it is possible to produce in stereoscopic photographs the still more beautiful effect of the sheen of water or of leaves.

The explanation of this curious phenomenon is as follows:—A dull surface, like unglazed white paper, reflects the light which falls on it equally in all directions, and, therefore, always looks equally bright, from whatever point it is seen; hence, of course, it appears equally bright to both eyes. On the other hand, a polished surface, beside the reflected light which it scatters equally in all directions, throws back other beams by *regular* reflection, which only pass in definite directions. Now one eve may receive this regularly reflected light and the other not; the surface will then appear much brighter to the one than to the other, and, as this can only happen with shining bodies, the effect of the black and white stereoscopic pictures appears like that of a polished surface.

Now if there were a complete combination of the impressions produced upon both retinæ, the union of

white and black would give grey. The fact, therefore, that when they are actually combined in the stereoscope they produce the effect of lustre, that is to say, an effect which cannot be produced by any kind of uniform grey surface, proves that the impressions on the two retinæ are not combined into one sensation.

That, again, this effect of stereoscopic lustre does not depend upon an alternation between the perceptions of the two eyes, on what is called the 'rivalry of the retinæ,' is proved by illuminating stereoscopic pictures for an instant with the electric spark. The same effect is perfectly produced.

In the third place, it can be proved, not only that the images received by the two eyes do not coalesce in our sensation, but that the two sensations which we receive from the two eyes are not exactly similar, that they can, on the contrary, be readily distinguished. For if the sensation given by the right eye were indistinguishably the same as that given by the left, it would follow that, at least in the case of the electric spark (when no movements of the eye can help us in distinguishing the two images), it would make no difference whether we saw the right hand stereoscopic picture with the right eye, and the left with the left, or put the two pictures into the stereoscope reversed, so as to see that intended for the right eye with the left, and that intended for the left eye with the right. But practically we find that it makes all the difference, for if we make the two pictures change places, the relief appears to be inverted: what should be further off seems nearer, what should stand out seems to fall back. Now, since when we look at objects by the momentary light of the electric spark, they always appear in their true relief and never reversed, it follows that the impression produced on the right eye is not indistinguishable from that on the left.

Lastly, there are some very curious and interesting phenomena seen when two pictures are put before the two eyes at the same time which cannot be combined so as to present the appearance of a single object. If, for example, we look with one eye at a page of print, and with the other at an engraving,[1] there follows what is called the 'rivalry' of the two fields of vision. The two images are not then seen at the same time, one covering the other; but at some points one prevails, and at others the other. If they are equally distinct, the places where one or the other appears usually change after a few seconds. But if the engraving presents anywhere in the field of vision a uniform white or black surface, then the printed letters which occupy the same position in the image presented to the other eye, will usually prevail exclusively over the uniform surface of the engraving. In spite, however, of what former observers have said to the contrary, I maintain that it is possible for the observer at any moment to control this rivalry by voluntary direction of his attention. If he tries to read the printed sheet, the letters remain visible, at least at the spot where for the moment he is reading. If, on the contrary, he tries to follow the outline and shadows of the engraving, then these prevail. I find, moreover, that it is possible to fix the attention upon a very feebly illuminated object, and make it prevail over a much brighter one, which coincides with it in the retinal image of the other eye. Thus, I can follow the watermarks of a white piece of paper and cease to see strongly-marked black figures in the other field. Hence the retinal rivalry is not a trial of strength between two sensations, but depends upon our fixing

[1] The practised observer is able to do this without any apparatus, but most persons will find it necessary to put the two objects in a stereoscope or, at least, to hold a book, or a sheet of paper, or the hand in front of the face, to serve for the partition in the stereoscope.—Tr.

on failing to fix the attention. Indeed there is scarcely any phenomenon so well fitted for the study of the causes which are capable of determining the attention. It is not enough to form the conscious intention of seeing first with one eye and then with the other; we must form as clear a notion as possible of what we expect to see. Then it will actually appear. If, on the other hand, we leave the mind at liberty without a fixed intention to observe a definite object, that alternation between the two pictures ensues which is called retinal rivalry. In this case, we find that, as a rule, bright and strongly marked objects in one field of vision prevail over those which are darker and less distinct in the other, either completely or at least for a time.

We may vary this experiment by using a pair of spectacles with different coloured glasses. We shall then find, on looking at the same objects with both eyes at once, that there ensues a similar rivalry between the two colours. Everything appears spotted over first with one and then with the other. After a time, however, the vividness of both colours becomes weakened, partly by the elements of the retina which are affected by each of them being tired, and partly by the complementary after-images which result. The alternation then ceases, and there ensues a kind of mixture of the two original colours.

It is much more difficult to fix the attention upon a colour than upon such an object as an engraving. For the attention upon which, as we have seen, the whole phenomenon of 'rivalry' depends, fixes itself with constancy only upon such a picture as continually offers something new for the eye to follow. But we may assist this by reflecting on the side of the glasses next the eye letters or other lines upon which the attention can fix. These reflected images themselves are not coloured, but as soon

as the attention is fixed upon one of them we become conscious of the colour of the corresponding glass.

These experiments on the rivalry of colours have given rise to a singular controversy among the best observers; and the possibility of such difference of opinion is an instructive hint as to the nature of the phenomenon itself. One party, including the names of Dove, Regnault,[1] Brücke, Ludwig,[2] Panum,[3] and Hering,[4] maintains that the result of a binocular view of two colours is the true combination-colour. Other observers, as Heinrich Meyer of Zürich, Volkmann, Meissner,[5] and Funke,[6] declare quite as positively that, under these conditions, they have never seen the combination-colour. I myself entirely agree with the latter, and a careful examination of the cases in which I might have imagined that I saw the combination-colour, has always proved to me that it was the result of phenomena of contrast. Each time that I brought the true combination-colour side by side with the binocular mixture of colours, the difference between the two was very apparent. On the other hand, there can of course be no doubt that the observers I first named really saw what they profess, so that there must here be great individual difference. Indeed with certain experiments which Dove recommends as particularly well fitted to prove the correctness of his conclusion, such as the binocular combination of complementary polarisation-colours into white, I could not myself see the slightest trace of a combination-colour.

[1] The distinguished French chemist, father of the well-known painter who was killed in the second siege of Paris.

[2] Professor of Physiology in the University of Leipzig.

[3] Professor of Physiology in the University of Kiel.

[4] Ewald Hering, Professor of Physiology in the University of Prague, lately in the Josephsakademie of Vienna.

[5] Professor of Physiology in the University of Göttingen.

[6] Professor of Physiology in the University of Freiburg.—Tr.

This striking difference in a comparatively simple observation seems to me to be of great interest. It is a remarkable confirmation of the supposition above made, in accordance with the empirical theory of vision, that in general only those sensations are perceived as separated in space, which can be separated one from another by voluntary movements. Even when we look at a compound colour with one eye, only three separate sensations are, according to Young's theory, produced together; but it is impossible to separate these by any movement of the eye, so that they always remain locally united. Yet we have seen that even in this case we may become conscious of a separation under certain circumstances; namely, when it is seen that part of the colour belongs to a transparent covering. When two corresponding points of the retinæ are illuminated with different colours, it will be rare for any separation between them to appear in ordinary vision; if it does, it will usually take place in the part of the field of sight outside the region of exact vision. But there is always a possibility of separating the compound impression thus produced into its two parts, which will appear to some extent independent of each other, and will move with the movements of the eye; and it will depend upon the degree of attention which the observer is accustomed to give to the region of indirect vision and to double images, whether he is able to separate the colours which fall on both retinæ at the same time. Mixed hues, whether looked at with one eye or with both, excite many simple sensations of colour at the same time, each having exactly the same position in the field of vision. The difference in the way in which such a compound-colour is regarded by different people depends upon whether this compound sensation is at once accepted as a coherent whole without any attempt at analysis, or whether the observer is able

by practice to recognise the parts of which it is composed, and to separate them from one another. The former is our usual (though not constant) habit when looking with one eye, while we are more inclined to the latter when using both. But inasmuch as this inclination must chiefly depend upon practice in observing distinctions, gained by preceding observation, it is easy to understand how great individual peculiarities may arise.

If we carefully observe the rivalry which ensues when we try to combine two stereoscopic drawings, one of which is in black lines on a white ground and the other in white lines on black, we shall see that the white and black lines which affect nearly corresponding points of each retina always remain visible side by side—an effect which of course implies that the white and black grounds are also visible. By this means the brilliant surface, which seems to shine like black lead, makes a much more stable impression than that produced under the operation of retinal rivalry by entirely different drawings. If we cover the lower half of the white figure on a black ground with a sheet of printed paper, the upper half of the combined stereoscopic image shows the phenomenon of Lustre, while in the lower we see Retinal Rivalry between the black lines of the figure and the black marks of the type. As long as the observer attends to the solid form of the object represented, the black and white outlines of the two stereoscopic drawings carry on in common the point of exact vision as it moves along them, and the effect can only be kept up by continuing to follow both. He must steadily keep his attention upon both drawings, and then the impression of each will be equally combined. There is no better way of preserving the combined effect of two stereoscopic pictures than this. Indeed it is possible to combine (at least partially and for a short

time) two entirely different drawings when put into the stereoscope, by fixing the attention upon the way in which they cover each other, watching, for instance, the angles at which their lines cross. But as soon as the attention turns from the angle to follow one of the lines which makes it, the picture to which the other line belongs vanishes

Let us now put together the results to which our inquiry into binocular vision has led us.

I. The excitement of corresponding points of the two retinæ is not indistinguishably combined into a single impression; for, if it were, it would be impossible to see Stereoscopic Lustre. And we have found reason to believe that this effect is not a consequence of Retinal Rivalry, even if we admit the latter phenomenon to belong to sensation at all, and not rather to the degree of attention. On the contrary the appearance of lustre is associated with the *restriction* of this rivalry.

II. The sensations which are produced by the excitation of corresponding points of each retina are not indistinguishably the same; for otherwise we should not be able to distinguish the true from the inverted or 'pseudoscopic' relief, when two stereoscopic pictures are illuminated by the electric spark.

III. The combination of the two different sensations received from corresponding retinal points is not produced by one of them being suppressed for a time; for, in the first place, the perception of solidity given by the two eyes depends upon our being at the same time conscious of the two different images, and, in the second, this perception of solidity is independent of any movement of the retinal images, since it is possible under momentary illumination.

We therefore learn that two distinct sensations are trans-

mitted from the two eyes, and reach the consciousness at the same time and without coalescing; that accordingly the combination of these two sensations into the single picture of the external world of which we are conscious in ordinary vision is not produced by any anatomical mechanism of sensation, but by a mental act.

IV. Further, we find that there is, on the whole, complete, or at least nearly complete, coincidence as to localisation in the field of vision of impressions of sight received from corresponding points of the retinæ; but that when we refer both impressions to the same object, their coincidence of localisation is much disturbed.

If this coincidence were the result of a direct function of sensation, it could not be disturbed by the mental operation which refers the two impressions to the same object. But we avoid the difficulty, if we suppose that the coincidence in localisation of the corresponding pictures received from the two eyes depends upon the power of measuring distances at sight which we gain by experience, that is, on an acquired knowledge of the meaning of the 'signs of localisation.' In this case it is simply one kind of experience opposing another; and we can then understand how the conclusion that two images belong to the same object should influence our estimation of their relative position by the measuring power of the eye, and how in consequence the distance of the two images from the fixed point in the field of vision should be regarded as the same, although it is not exactly so in reality.

But if the practical coincidence of corresponding points as to localisation in the two fields of vision does not depend upon sensation, it follows that the original power of comparing different distances in each separate field of vision cannot depend upon direct sensation. For, if it were so, it would follow that the coincidence of the two

fields would be completely established by direct sensation, as soon as the observer had got his two fixed points to coincide and a single meridian of one eye to coincide with the corresponding one of the other.

The reader sees how this series of facts has driven us by force to the empirical theory of vision. It is right to mention that lately fresh attempts have been made to explain the origin of our perception of solidity and the phenomena of single and double binocular vision by the assumption of some ready-made anatomical mechanism. We cannot criticise these attempts here: it would lead us too far into details. Although many of these hypotheses are very ingenious (and at the same time very indefinite and elastic), they have hitherto always proved insufficient; because the actual world offers us far more numerous relations than the authors of these attempts could provide for. Hence, as soon as they have arranged one of their systems to explain any particular phenomenon of vision, it is found not to answer for any other. Then, in order to help out the hypothesis, the very doubtful assumption has to be made that, in these other cases, sensation is overcome and extinguished by opposing experience. But what confidence could we put in any of our perceptions if we were able to extinguish our sensations as we please, whenever they concern an object of our attention, for the sake of previous conceptions to which they are opposed? At any rate, it is clear that in every case where experience must finally decide, we shall succeed much better in forming a correct notion of what we see, if we have no opposing sensations to overcome, than if a correct judgment must be formed in spite of them.

It follows that the hypotheses which have been successively framed by the various supporters of intuitive

theories of vision, in order to suit one phenomenon after another, are really quite unnecessary. No fact has yet been discovered inconsistent with the Empirical Theory: which does not assume any peculiar modes of physiological action in the nervous system, nor any hypothetical anatomical structures; which supposes nothing more than the well known association between the impressions we receive and the conclusions we draw from them, according to the fundamental laws of daily experience. It is true that we cannot at present offer any complete scientific explanation of the mental operations involved, and there is no immediate prospect of our doing so. But since these operations actually exist, and since hitherto every form of the intuitive theory has been obliged to fall back on their reality when all other explanation failed, these mysteries of the laws of thought cannot be regarded from a scientific point of view as constituting any deficiency in the empirical theory of vision.

It is impossible to draw any line in the study of our perceptions of space which shall sharply separate those which belong to direct Sensation from those which are the result of Experience. If we attempt to draw such a boundary, we find that experience proves more minute, more direct and more exact than supposed sensation, and in fact proves its own superiority by overcoming the latter. The only supposition which does not lead to any contradiction is that of the Empirical Theory, which regards all our perceptions of space as depending upon experience, and not only the qualities, but even the local signs of the sense of sight as nothing more than signs, the meaning of which we have to learn by experience.

We become acquainted with their meaning by comparing them with the result of our own movements, with

he changes which we thus produce in the outer world. The infant first begins to play with its hands. There is a time when it does not know how to turn its eyes or its hands to an object which attracts its attention by its brightness or colour. When a little older, a child seizes whatever is presented to it, turns it over and over again, looks at it, touches it, and puts it in his mouth. The simplest objects are what a child likes best, and he always prefers the most primitive toy to the elaborate inventions of modern ingenuity. After he has looked at such a toy every day for weeks together, he learns at last all the perspective images which it presents; then he throws it away and wants a fresh toy to handle like the first. By this means the child learns to recognise the different views which the same object can afford, in connection with the movements which he is constantly giving it. The conception of the shape of any object, gained in this manner, is the result of associating all these visual images. When we have obtained an accurate conception of the form of any object, we are then able to imagine what appearance it would present, if we looked at it from some other point of view. All these different views are combined in the judgment we form as to the dimensions and shape of an object. And, consequently, when we are once acquainted with this, we can deduce from it the various images it would present to the sight when seen from different points of view, and the various movements which we should have to impress upon it in order to obtain these successive images.

I have often noticed a striking instance of what I have been saying in looking at stereoscopic pictures. If, for example, we look at elaborate outlines of complicated crystalline forms, it is often at first difficult to see what they mean. When this is the case, I look out two points

in the diagram which correspond, and make them overlap by a voluntary movement of the eyes. But as long as I have not made out what kind of form the drawings are intended to represent, I find that my eyes begin to diverge again, and the two points no longer coincide. Then I try to follow the different lines of the figure, and suddenly I see what the form represented is. From that moment my two eyes pass over the outlines of the apparently solid body with the utmost ease, and without ever separating. As soon as we have gained a correct notion of the shape of an object, we have the rule for the movements of the eyes which are necessary for seeing it. In carrying out these movements, and thus receiving the visual impressions we expect, we retranslate the notion we have formed into reality, and by finding this retranslation agrees with the original, we become convinced of the accuracy of our conception.

This last point is, I believe, of great importance. The meaning we assign to our sensations depends upon experiment, and not upon mere observation of what takes place around us. We learn by experiment that the correspondence between two processes takes place at any moment that we choose, and under conditions which we can alter as we choose. Mere observation would not give us the same certainty, even though often repeated under different conditions. For we should thus only learn that the processes in question appear together frequently (or even always, as far as our experience goes); but mere observation would not teach us that they appear together at any moment we select.

Even in considering examples of scientific observation, methodically carried out, as in astronomy, meteorology, or geology, we never feel fully convinced of the causes of the phenomena observed until we can demonstrate the working of these same forces by actual experiment in

the laboratory. So long as science is not experimental it does not teach us the knowledge of any new force.[1]

It is plain that, by the experience which we collect in the way I have been describing, we are able to learn as much of the meaning of sensible 'signs' as can afterwards be verified by further experience; that is to say, all that is real and positive in our conceptions.

It has been hitherto supposed that the sense of *touch* confers the notion of space and movement. At first of course the only direct knowledge we acquire is that we can produce, by an act of volition, changes of which we are cognisant by means of touch and sight. Most of these voluntary changes are movements, or changes in the relations of space; but we can also produce changes in an object itself. Now, can we recognise the movements of our hands and eyes as changes in the relations of space, without knowing it beforehand? and can we distinguish them from other changes which affect the properties of external objects? I believe we can. It is an essentially distinct character of the Relations of Space that they are *changeable relations between objects which do not depend on their quality or quantity*, while all other material relations between objects depend upon their properties. The perceptions of sight prove this directly and easily. A movement of the eye which causes the retinal image to shift its place upon the retina always produces the same series of changes as often as it is repeated, whatever objects the field of vision may contain. The effect is that the impressions which had before the local signs a_0, a_1, a_2, a_3, receive the new local signs b_0, b_1, b_2, b_3; and this may always occur in the same

[1] An interesting paper, applying this view of the 'experimental' character of progressive science to Zoology, has been published by M. Lacaze Duthiers, in the first number of his *Archives de Zoologie*.—Tr.

way, whatever be the quality of the impressions. By this means we learn to recognise such changes as belonging to the special phenomena which we call changes in space. This is enough for the object of Empirical Philosophy, and we need not further enter upon a discussion of the question, how much of universal conceptions of space is derived *a priori*, and how much *a posteriori*?[1]

An objection to the empirical Theory of Vision might be found in the fact that illusions of the senses are possible; for if we have learnt the meaning of our sensations from experience, they ought always to agree with experience. The explanation of the possibility of illusions lies in the fact that we transfer the notions of external objects, which would be correct under normal conditions, to cases in which unusual circumstances have altered the retinal pictures. What I call 'observation under normal conditions' implies not only that the rays of light must pass in straight lines from each visible point to the cornea, but also that we must use our eyes in the way they should be used in order to receive the clearest and most easily distinguishable images. This implies that we should successively bring the images of the separate points of the outline of the objects we are looking at upon the centres of both retinæ (the yellow spot), and also move the eyes so as to obtain the surest comparison between their various positions. Whenever we deviate from these conditions of normal vision, illusions are the result. Such are the long recognised effects of the refraction or reflection of rays of light before they enter the eye. But there are many other

[1] The question of the origin of our conceptions of space is discussed by Mr. Bain on empirical principles in his treatise on *The Senses and the Intellect*, pp. 114–118, 189–194, 245, 363–392, &c.—Tr.

causes of mistake as to the position of the objects we see—defective accommodation when looking through one or two small openings, improper convergence when looking with one eye only, irregular position of the eye-ball from external pressure or from paralysis of its muscles. Moreover, illusions may come in from certain elements of sensation not being accurately distinguished; as, for instance, the degree of convergence of the two eyes, of which it is difficult to form an accurate judgment when the muscles which produce it become fatigued.

The simple rule for all illusions of sight is this: *we always believe that we see such objects as would, under conditions of normal vision, produce the retinal image of which we are actually conscious.* If these images are such as could not be produced by any normal kind of observation, we judge of them according to their nearest resemblance; and in forming this judgment, we more easily neglect the parts of sensation which are imperfectly than those which are perfectly apprehended. When more than one interpretation is possible, we usually waver involuntarily between them; but it is possible to end this uncertainty by bringing the idea of any of the possible interpretations we choose as vividly as possible before the mind by a conscious effort of the will.

These illusions obviously depend upon mental processes which may be described as false inductions. But there are, no doubt, judgments which do not depend upon our consciously thinking over former observations of the same kind, and examining whether they justify the conclusion which we form. I have, therefore, named these 'unconscious judgments;' and this term, though accepted by other supporters of the empirical theory, has excited much opposition, because, according to generally-accepted psychological doctrines, a *judgment,*

or *logical conclusion*, is the culminating point of the conscious operations of the mind. But the judgments which play so great a part in the perceptions we derive from our senses cannot be expressed in the ordinary form of logically analysed conclusions, and it is necessary to deviate somewhat from the beaten paths of psychological analysis in order to convince ourselves that we really have here the same kind of mental operation as that involved in conclusions usually recognised as such. There appears to me to be in reality only a superficial difference between the 'conclusions' of logicians and those inductive conclusions of which we recognise the result in the conceptions we gain of the outer world through our sensations. The difference chiefly depends upon the former conclusions being capable of expression in words, while the latter are not; because, instead of words, they only deal with sensations and the memory of sensations. Indeed, it is just the impossibility of describing sensations, whether actual or remembered, in words, which makes it so difficult to discuss this department of psychology at all.

Beside the knowledge which has to do with Notions, and is, therefore, capable of expression in words, there is another department of our mental operations, which may be described as knowledge of the relations of those impressions on the senses which are not capable of direct verbal expression. For instance, when we say that we 'know'[1] a man, a road, a fruit, a perfume, we mean that we have seen, or tasted, or smelt, these objects. We keep the sensible impression fast in our memory, and we shall recognise it again when it is repeated, but we

[1] In German this kind of knowledge is expressed by the verb *kennen* (*cognoscere, connaître*), to be acquainted with, while *wissen* (*scire, savoir*) means to be aware of. The former kind of knowledge is only applicable to objects directly cognisable by the senses, whereas the latter applies to notions or conceptions which can be formally stated as propositions.—Tr.

cannot describe the impression in words, even to ourselves. And yet it is certain that this kind of knowledge (*Kennen*) may attain the highest possible degree of precision and certainty, and is so far not inferior to any knowledge (*Wissen*) which can be expressed in words; but it is not directly communicable, unless the object in question can be brought actually forward, or the impression it produces can be otherwise represented —as by drawing the portrait of a man instead of producing the man himself.

It is an important part of the former kind of knowledge to be acquainted with the particular innervation of muscles, which is necessary in order to produce any effect we intend by moving our limbs. As children, we must learn to walk; we must afterwards learn how to skate or go on stilts, how to ride, or swim, or sing, or pronounce a foreign language. Moreover, observation of infants shows that they have to learn a number of things which afterwards they will know so well as entirely to forget that there was ever a time when they were ignorant of them. For example, everyone of us had to learn, when an infant, how to turn his eyes toward the light in order to see. This kind of 'knowledge' (*Kennen*) we also call 'being able' to do a thing (*können*), or 'understanding' how to do it (*verstehen*), as, 'I know how to ride,' 'I am able to ride,' or 'I understand how to ride.'[1]

It is important to notice that this 'knowledge' of the effort of the will to be exerted must attain the highest possible degree of certainty, accuracy, and precision, for us to be able to maintain so artificial a balance as is necessary for walking on stilts or for skating, for the singer to know how to strike a note with his voice, or the

[1] The German word *können* is said to be of the same etymology as *kennen*, and so their likeness in form would be explained by their likeness in meaning.

violin-player with his finger, so exactly that its vibration shall not be out by a hundredth part.

Moreover, it is clearly possible, by using these sensible images of memory instead of words, to produce the same kind of combination which, when expressed in words, would be called a proposition or a conclusion. For example, I may know that a certain person with whose face I am familiar, has a peculiar voice, of which I have an equally lively recollection. I should be able with the utmost certainty to recognise his face and his voice among a thousand, and each would recall the other. But I cannot express this fact in words, unless I am able to add some other characters of the person in question which can be better defined. Then I should be able to resort to a syllogism and say, 'This voice which I now hear belongs to the man whom I saw then and there.' But universal, as well as particular conclusions, may be expressed in terms of sensible impressions, instead of words. To prove this I need only refer to the effect of works of art. The statue of a god would not be capable of conveying a notion of a definite character and disposition, if I did not know that the form of face and the expression it wears have usually or constantly a certain definite signification. And, to keep in the domain of the perceptions of the senses, if I know that a particular way of looking, for which I have learnt how to employ exactly the right kind of innervation, is necessary in order to bring into direct vision a point two feet off and so many feet to the right, this also is a universal proposition which applies to every case in which I have fixed a given point at that distance before, or may do so hereafter. It is a piece of knowledge which cannot be expressed in words, but is the result which sums up my previous successful experience. It may at any moment become the *major* premiss of a syllogism, whenever, in fact, I fix a point in the supposed position and feel that I

do so by looking as that major proposition states. This perception of what I am doing is my *minor* proposition, and the '*conclusion*' is that the object I am looking for will be found at the spot in question.

Suppose that I employ the same way of looking, but look into a stereoscope. I am now aware that there is no real object before me at the spot I am looking at; but I have the same sensible impression as if one were there; and yet I am unable to describe this impression to myself or others, or to characterise it otherwise than as 'the same impression which would arise in the normal method of observation, if an object were really there.' It is important to notice this. No doubt the physiologist can describe the impression in other ways, by the direction of the eyes, the position of the retinal images, and so on; but there is no other way of directly defining and characterising the sensation which we experience. Thus we may recognise it as an illusion, but yet we cannot get rid of the sensation of this illusion; for we cannot extinguish our remembrance of its normal signification, even when we know that in the case before us this does not apply—just as little as we are able to drive out of the mind the meaning of a word in our mother tongue, when it is employed as a sign for an entirely different purpose.

These conclusions in the domain of our sensible perceptions appear as inevitable as one of the forces of nature, and hence their results seem to be directly apprehended, without any effort on our part; but this does not distinguish them from logical and conscious conclusions, or at least from those which really deserve the name. All that we can do by voluntary and conscious effort, in order to come to a conclusion, is, after all, only to supply complete materials for constructing the necessary premisses. As soon as this is done, the conclusion forces itself upon us. Those conclusions which (it is

supposed) may be accepted or avoided as we please, are not worth much.

The reader will see that these investigations have led us to a field of mental operations which has been seldom entered by scientific explorers. The reason is that it is difficult to express these operations in words. They have been hitherto most discussed in writings on æsthetics, where they play an important part as Intuition, Unconscious Ratiocination, Sensible Intelligibility, and such obscure designations. There lies under all these phrases the false assumption that the mental operations we are discussing take place in an undefined, obscure, half-conscious fashion; that they are, so to speak, mechanical operations, and thus subordinate to conscious thought, which can be expressed in language. I do not believe that any difference in kind between the two functions can be proved. The enormous superiority of knowledge which has become ripe for expression in language, is sufficiently explained by the fact that, in the first place, speech makes it possible to collect together the experience of millions of individuals and thousands of generations, to preserve them safely, and by continual verification to make them gradually more and more certain and universal; while, in the second place, all deliberately combined actions of mankind, and so the greatest part of human power, depend on language. In neither of these respects can mere familiarity with phenomena (*das Kennen*) compete with the knowledge of them which can be communicated by speech (*das Wissen*); and yet it does not follow of necessity that the one kind of knowledge should be of a different nature from the other, or less clear in its operation.

The supporters of Intuitive Theories of Sensation often appeal to the capabilities of new-born animals, many of

which show themselves much more skilful than a human infant. It is quite clear that an infant, in spite of the greater size of its brain, and its power of mental development, learns with extreme slowness to perform the simplest tasks; as, for example, to direct its eyes to an object or to touch what it sees with its hands. Must we not conclude that a child has much more to learn than an animal which is safely guided, but also restricted, by its instincts? It is said that the calf sees the udder and goes after it, but it admits of question whether it does not simply smell it, and make those movements which bring it nearer to the scent.[1] At any rate, the child knows nothing of the meaning of the visual image presented by its mother's breast. It often turns obstinately away from it to the wrong side and tries to find it there. The young chicken very soon pecks at grains of corn, but it pecked while it was still in the shell, and when it hears the hen peck, it pecks again, at first seemingly at random. Then, when it has by chance hit upon a grain, it may, no doubt, learn to notice the field of vision which is at the moment presented to it. The process is all the quicker because the whole of the mental furniture which it requires for its life is but small.

We need, however, further investigations on the subject in order to throw light upon this question. As far as the observations with which I am acquainted go, they do not seem to me to prove that anything more than certain tendencies is born with animals. At all events one distinction between them and man lies precisely in this, that these innate or congenital tendencies, impulses or instincts are in him reduced to the smallest possible number and strength.[2]

[1] See Darwin on the *Expression of the Emotions*, p. 47.—Tr.

[2] See on this subject Bain on the *Senses and the Intellect*, p. 293; also a paper on 'Instinct' in *Nature*, Oct. 10, 1872.

There is a most striking analogy between the entire range of processes which we have been discussing, and another System of Signs, which is not given by nature but arbitrarily chosen, and which must undoubtedly be learned before it is understood. I mean the words of our mother tongue.

Learning how to speak is obviously a much more difficult task than acquiring a foreign language in after life. First, the child has to guess that the sounds it hears are intended to be signs at all; next, the meaning of each separate sound must be found out, by the same kind of induction as the meaning of the sensations of sight or touch; and yet we see children by the end of their first year already understanding certain words and phrases, even if they are not yet able to repeat them. We may sometimes observe the same in dogs.

Now this connection between Names and Objects, which demonstrably must be *learnt*, becomes just as firm and indestructible as that between Sensations and the Objects which produce them. We cannot help thinking of the usual signification of a word, even when it is used exceptionably in some other sense; we cannot help feeling the mental emotions which a fictitious narrative calls forth, even when we know that it is not true; just in the same way as we cannot get rid of the normal signification of the sensations produced by any illusion of the senses, even when we know that they are not real.

There is one other point of comparison which is worth notice. The elementary signs of language are only twenty-six letters, and yet what wonderfully varied meanings can we express and communicate by their combination! Consider, in comparison with this, the enormous number of elementary signs with which the machinery of sight is provided. We may take the number of fibres in the optic nerves as two hundred and fifty thousand. Each

of these is capable of innumerable different degrees of sensation of one, two, or three primary colours. It follows that it is possible to construct an immeasurably greater number of combinations here than with the few letters which build up our words. Nor must we forget the extremely rapid changes of which the images of sight are capable. No wonder, then, if our senses speak to us in language which can express far more delicate distinctions and richer varieties than can be conveyed by words.

This is the solution of the riddle of how it is possible to see; and, as far as I can judge, it is the only one of which the facts at present known admit. Those striking and broad incongruities between Sensations and Objects, both as to quality and to localisation, on which we dwelt, are just the phenomena which are most instructive; because they compel us to take the right road. And even those physiologists who try to save fragments of a pre-established harmony between sensations and their objects, cannot but confess that the completion and refinement of sensory perceptions depend so largely upon experience, that it must be the latter which finally decides whenever they contradict the supposed congenital arrangements of the organ. Hence the utmost significance which may still be conceded to any such anatomical arrangements is that they are possibly capable of helping the first practice of our senses.

The correspondence, therefore, between the external world and the Perceptions of Sight rests, either in whole or in part, upon the same foundation as all our knowledge of the actual world—on *experience*, and on constant *verification* of its accuracy by experiments which we perform with every movement of our body. It follows, of course, that we are only warranted in accepting the reality of this correspondence so far as these means of

verification extend, which is really as far as for practical purposes we need.

Beyond these limits, as, for example, in the region of Qualities, we are in some instances able to prove conclusively that there is no correspondence at all between sensations and their objects.

Only the relations of time, of space, of equality, and those which are derived from them, of number, size, regularity of coexistence and of sequence—'mathematical relations' in short—are common to the outer and the inner world, and here we may indeed look for a complete correspondence between our conceptions and the objects which excite them.

But it seems to me that we should not quarrel with the bounty of nature because the greatness, and also the emptiness, of these abstract relations have been concealed from us by the manifold brilliance of a system of signs; since thus they can be the more easily surveyed and used for practical ends, while yet traces enough remain visible to guide the philosophical spirit aright, in its search after the meaning of sensible Images and Signs.

ON THE CONSERVATION OF FORCE.

INTRODUCTION TO A SERIES OF LECTURES DELIVERED AT CARLSRUHE IN THE WINTER OF 1862-1863.

As I have undertaken to deliver here a series of lectures, I think the best way in which I can discharge that duty will be to bring before you, by means of a suitable example, some view of the special character of those sciences to the study of which I have devoted myself. The natural sciences, partly in consequence of their practical applications, and partly from their intellectual influence on the last four centuries, have so profoundly, and with such increasing rapidity, transformed all the relations of the life of civilised nations; they have given these nations such increase of riches, of enjoyment of life, of the preservation of health, of means of industrial and of social intercourse, and even such increase of political power, that every educated man who tries to understand the forces at work in the world in which he is living, even if he does not wish to enter upon the study of a special science, must have some interest in that peculiar kind of mental labour which works and acts in the sciences in question.

On a former occasion I have already discussed the characteristic differences which exist between the natural and the mental sciences as regards the kind of scientific work. I then endeavoured to show that it is more

especially in the thorough conformity with law which natural phenomena and natural products exhibit, and in the comparative ease with which laws can be stated, that this difference exists. Not that I wish by any means to deny, that the mental life of individuals and peoples is also in conformity with law, as is the object of philosophical, philological, historical, moral, and social sciences to establish. But in mental life, the influences are so interwoven, that any definite sequence can but seldom be demonstrated. In Nature the converse is the case. It has been possible to discover the law of the origin and progress of many enormously extended series of natural phenomena with such accuracy and completeness that we can predict their future occurrence with the greatest certainty; or in cases in which we have power over the conditions under which they occur, we can direct them just according to our will. The greatest of all instances of what the human mind can effect by means of a well-recognised law of natural phenomena is that afforded by modern astronomy. The one simple law of gravitation regulates the motions of the heavenly bodies not only of our own planetary system, but also of the far more distant double stars; from which, even the ray of light, the quickest of all messengers, needs years to reach our eye; and just on account of this simple conformity with law, the motions of the bodies in question, can be accurately predicted and determined both for the past and for future years and centuries to a fraction of a minute.

On this exact conformity with law depends also the certainty with which we know how to tame the impetuous force of steam, and to make it the obedient servant of our wants. On this conformity depends, moreover, the intellectual fascination which chains the physicist to his subjects It is an interest of quite a different kind to that

which mental and moral sciences afford. In the latter it is man in the various phases of his intellectual activity who chains us. Every great deed of which history tells us, every mighty passion which art can represent, every picture of manners, of civic arrangements, of the culture of peoples of distant lands, or of remote times, seizes and interests us, even if there is no exact scientific connection among them. We continually find points of contact and comparison in our own conceptions and feelings; we get to know the hidden capacities and desires of the mind, which in the ordinary peaceful course of civilised life remain unawakened.

It is not to be denied that, in the natural sciences, this kind of interest is wanting. Each individual fact, taken of itself, can indeed arouse our curiosity or our astonishment, or be useful to us in its practical applications. But intellectual satisfaction we obtain only from a connection of the whole, just from its conformity with law. *Reason* we call that faculty innate in us of discovering laws and applying them with thought. For the unfolding of the peculiar forces of pure reason in their entire certainty and in their entire bearing, there is no more suitable arena than inquiry into nature in the wider sense, the mathematics included. And it is not only the pleasure at the successful activity of one of our most essential mental powers; and the victorious subjections to the power of our thought and will of an external world, partly unfamiliar, and partly hostile, which is the reward of this labour; but there is a kind, I might almost say, of artistic satisfaction, when we are able to survey the enormous wealth of Nature as a regularly-ordered whole — a kosmos, an image of the logical thought of our own mind.

The last decades of scientific development have led us to the recognition of a new universal law of all natural phenomena, which, from its extraordinarily extended range,

and from the connection which it constitutes between natural phenomena of all kinds, even of the remotest times and the most distant places, is especially fitted to give us an idea of what I have described as the character of the natural sciences, which I have chosen as the subject of this lecture.

This law is *the Law of the Conservation of Force*, a term the meaning of which I must first explain. It is not absolutely new; for individual domains of natural phenomena it was enunciated by Newton and Daniel Bernoulli; and Rumford and Humphry Davy have recognised distinct features of its presence in the laws of heat.

The possibility that it was of universal application was first stated by Dr. Julius Robert Mayer, a Schwabian physician (now living in Heilbronn) in the year 1842, while almost simultaneously with, and independently of him, James Prescot Joule, an English manufacturer, made a series of important and difficult experiments on the relation of heat to mechanical force, which supplied the chief points in which the comparison of the new theory with experience was still wanting.

The law in question asserts, that the *quantity of force which can be brought into action in the whole of Nature is unchangeable*, and can neither be increased nor diminished. My first object will be to explain to you what is understood by *quantity of force*; or as the same idea is more popularly expressed with reference to its technical application, what we call *amount of work* in the mechanical sense of the word.

The idea of work for machines, or natural processes, is taken from comparison with the working power of man; and we can therefore best illustrate from human labour, the most important features of the question with which we are concerned. In speaking of the work of machines, and of natural forces, we must, of course, in this compari-

son eliminate anything in which activity of intelligence comes into play. The latter is also capable of the hard and intense work of thinking, which tries a man just as muscular exertion does. But whatever of the actions of intelligence is met with in the work of machines, of course is due to the mind of the constructor and cannot be assigned to the instrument at work.

Now, the external work of man is of the most varied kind as regards the force or ease, the form and rapidity, of the motions used on it, and the kind of work produced. But both the arm of the blacksmith who delivers his powerful blows with the heavy hammer, and that of the violinist who produces the most delicate variations in sound, and the hand of the lace-maker who works with threads so fine that they are on the verge of the invisible, all these acquire the force which moves them in the same manner and by the same organs, namely, the muscles of the arm. An arm the muscles of which are lamed is incapable of doing any work; the moving force of the muscle must be at work in it, and these must obey the nerves, which bring to them orders from the brain. That member is then capable of the greatest variety of motions; it can compel the most varied instruments to execute the most diverse tasks.

Just so is it with machines: they are used for the most diversified arrangements. We produce by their agency an infinite variety of movements, with the most various degrees of force and rapidity, from powerful steam-hammers and rolling-mills, where gigantic masses of iron are cut and shaped like butter, to spinning and weaving-frames, the work of which rivals that of the spider. Modern mechanism has the richest choice of means of transferring the motion of one set of rolling wheels to another with greater or less velocity; of changing the rotating motion of wheels into the up-and-down motion

of the piston-rod, of the shuttle, of falling hammers and stamps; or, conversely, of changing the latter into the former; or it can, on the other hand, change movements of uniform into those of varying velocity, and so forth. Hence this extraordinarily rich utility of machines for so extremely varied branches of industry. But one thing is common to all these differences; they all need a *moving force*, which sets and keeps them in motion, just as the works of the human hand all need the moving force of the muscles.

Now, the work of the smith requires a far greater and more intense exertion of the muscles than that of the violin-player; and there are in machines corresponding differences in the power and duration of the moving force required. These differences, which correspond to the different degree of exertion of the muscles in human labour, are alone what we have to think of when we speak of the *amount of work* of a machine. We have nothing to do here with the manifold character of the actions and arrangements which the machines produce; we are only concerned with an expenditure of force.

This very expression which we use so fluently, 'expenditure of force,' which indicates that the force applied has been expended and lost, leads us to a further characteristic analogy between the effects of the human arm and those of machines. The greater the exertion, and the longer it lasts, the more is the arm *tired*, and the more *is the store of its moving force for the time exhausted.* We shall see that this peculiarity of becoming exhausted by work is also met with in the moving forces of inorganic nature; indeed, that this capacity of the human arm of being tired is only one of the consequences of the law with which we are now concerned. When fatigue sets in, recovery is needed, and this can only be effected by rest and nourishment. We shall find that also in the

inorganic moving forces, when their capacity for work is spent, there is a possibility of reproduction, although in general other means must be used to this end than in the case of the human arm.

From the feeling of exertion and fatigue in our muscles, we can form a general idea of what we understand by amount of work; but we must endeavour, instead of the indefinite estimate afforded by this comparison, to form a clear and precise idea of the standard by which we have to measure the amount of work. This we can do better by the simplest inorganic moving forces than by the actions of our muscles, which are a very complicated apparatus, acting in an extremely intricate manner.

Let us now consider that moving force which we know best, and which is simplest—gravity. It acts, for example as such, in those clocks which are driven by a weight. This weight fastened to a string, which is wound round a pulley connected with the first toothed wheel of the clock, cannot obey the pull of gravity without setting the whole clockwork in motion. Now I must beg you to pay special attention to the following points: the weight cannot put the clock in motion without itself sinking; did the weight not move, it could not move the clock, and its motion can only be such a one as obeys the action of gravity. Hence, if the clock is to go, the weight must continually sink lower and lower, and must at length sink so far that the string which supports it is run out. The clock then stops. The useful effect of its weight is for the present exhausted. Its gravity is not lost or diminished; it is attracted by the earth as before, but the capacity of this gravity to produce the motion of the clockwork is lost. It can only keep the weight at rest in the lowest point of its path, it cannot farther put it in motion.

But we can wind up the clock by the power of the arm, by which the weight is again raised. When this has been

done, it has regained its former capacity, and can again set the clock in motion.

We learn from this that a raised weight possesses a *moving force*, but that it must necessarily sink if this force is to act; that by sinking, this moving force is exhausted, but by using another extraneous moving force —that of the arm—its activity can be restored.

The work which the weight has to perform in driving the clock is not indeed great. It has continually to overcome the small resistances which the friction of the axles and teeth, as well as the resistance of the air, oppose to the motion of the wheels, and it has to furnish the force for the small impulses and sounds which the pendulum produces at each oscillation. If the weight is detached from the clock, the pendulum swings for a while before coming to rest, but its motion becomes each moment feebler, and ultimately ceases entirely, being gradually used up by the small hindrances I have mentioned. Hence, to keep the clock going, there must be a moving force, which, though small, must be continually at work. Such a one is the weight.

We get, moreover, from this example, a measure for the amount of work. Let us assume that a clock is driven by a weight of a pound, which falls five feet in twenty-four hours. If we fix ten such clocks, each with a weight of one pound, then ten clocks will be driven twenty-four hours; hence, as each has to overcome the same resistances in the same time as the others, ten times as much work is performed for ten pounds fall through five feet. Hence, we conclude that the height of the fall being the same, the work increases directly as the weight.

Now, if we increase the length of the string so that the weight runs down ten feet, the clock will go two days instead of one; and, with double the height of fall, the weight will overcome on the second day the same

resistances as on the first, and will therefore do twice as much work as when it can only run down five feet. The weight being the same, the work increases as the height of fall. Hence, we may take the product of the weight into the height of fall as a measure of work, at any **rate,** in the present case. The application of this measure is, in fact, not limited to the individual case, but the universal standard adopted in manufactures for measuring magnitude of work is a *foot pound*—that is, the amount of work which a pound raised through a foot can produce.[1]

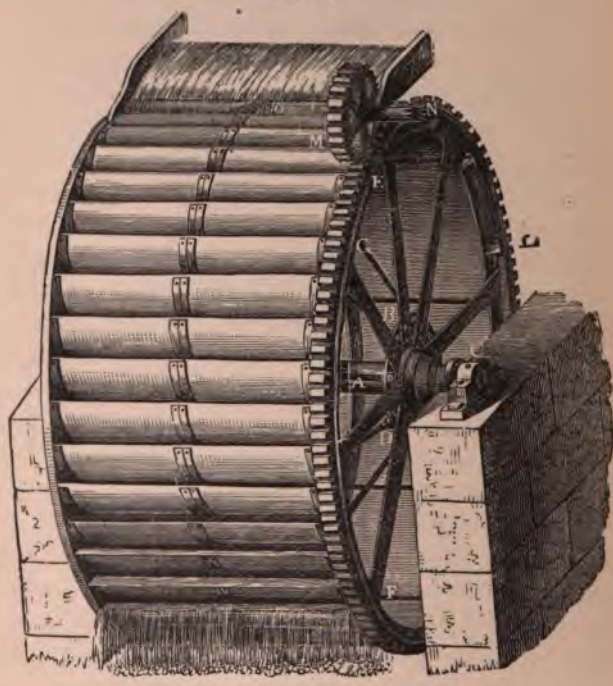
We may apply this measure of work to all kinds of machines, for we should be able to set them all in motion by means of a weight sufficient to turn a pulley. We could thus always express the magnitude of any driving force, for any given machine, by the magnitude and height of fall of such a weight as would be necessary to keep the machine going with its arrangements until it had performed a certain work. Hence it is that the measurement of work by foot pounds is universally applicable. The use of such a weight as a driving force would not indeed be practically advantageous in those cases in which we were compelled to raise it by the power of our own arm; it would in that case be simpler to work the machine by the direct action of the arm. In the clock we use a weight so that we need not stand the whole day at the clockwork, as we should have to do to move it directly. By winding up the clock we accumulate a store of working capacity in it, which is sufficient for the expenditure of the next twenty-four hours.

The case is somewhat different when Nature herself raises the weight, which then works for us. She does not do this with solid bodies, at least not with such regularity as to be utilised; but she does it abundantly with water,

[1] This is the *technical* measure of work; to convert it into scientific measure it must be multiplied by the intensity of gravity.

which, being raised to the tops of mountains by meteorological processes, returns in streams from them. The gravity of water we use as moving force, the most direct application being in what are called *overshot* wheels, one of which is represented in Fig. 38. Along the circumference of such a wheel are a series of buckets, which act as

Fig. 38.



receptacles for the water, and, on the side turned to the observer, have the tops uppermost; on the opposite side the tops of the buckets are upside-down. The water flows at M into the buckets of the front of the wheel, and at F, where the mouth begins to incline downwards, it flows out. The buckets on the circumference are filled on the

side turned to the observer, and empty on the other side. Thus the former are weighted by the water contained in them, the latter not; the weight of the water acts continuously on only one side of the wheel, draws this down, and thereby turns the wheel; the other side of the wheel offers no resistance, for it contains no water. It is thus the weight of the falling water which turns the wheel, and furnishes the motive power. But you will at once see that the mass of water which turns the wheel must necessarily fall in order to do so, and that though, when it has reached the bottom, it has lost none of its gravity, it is no longer in a position to drive the wheel, if it is not restored to its original position, either by the power of the human arm or by means of some other natural force. If it can flow from the mill-stream to still lower levels, it may be used to work other wheels. But when it has reached its lowest level, the sea, the last remainder of the moving force is used up, which is due to gravity—that is, to the attraction of the earth, and it cannot act by its weight until it has been again raised to a high level. As this is actually effected by meteorological processes, you will at once observe that these are to be considered as sources of moving force.

Water-power was the first inorganic force which man learnt to use instead of his own labour or of that of domestic animals. According to Strabo, it was known to King Mithridates, of Pontus, who was also otherwise celebrated for his knowledge of nature; near his palace there was a water-wheel. Its use was first introduced among the Romans in the time of the first Emperors. Even now we find water-mills in all mountains, valleys, or wherever there are rapidly-flowing, regularly-filled, brooks and streams. We find water-power used for all purposes which can possibly be effected by machines. It drives mills which grind corn, saw-mills, hammers and oil-presses,

spinning-frames and looms, and so forth. It is the cheapest of all motive powers, it flows spontaneously from the inexhaustible stores of nature; but it is restricted to a particular place, and only in mountainous countries is it present in any quantity; in level countries extensive reservoirs are necessary for damming the rivers to produce any amount of water-power.

Before passing to the discussion of other motive forces, I must answer an objection which may readily suggest itself. We all know that there are numerous machines, systems of pulleys, levers and cranes, by the aid of which heavy burdens may be lifted by a comparatively small expenditure of force. We have all of us often seen one or two workmen hoist heavy masses of stones to great heights, which they would be quite unable to do directly; in like manner, one or two men, by means of a crane, can transfer the largest and heaviest chests from a ship to the quay. Now it may be asked, If a large, heavy weight had been used for driving a machine, would it not be very easy, by means of a crane or a system of pulleys, to raise it anew, so that it could again be used as a motor, and thus acquire motive power, without being compelled to use a corresponding exertion in raising the weight?

The answer to this is, that all these machines, in that degree in which for the moment they facilitate the exertion, also prolong it, so that by their help no motive power is ultimately gained. Let us assume that four labourers have to raise a load of four hundredweight, by means of a rope passing over a single pulley. Every time the rope is pulled down through four feet, the load is also raised through four feet. But now, for the sake of comparison, let us suppose the same load hung to a block of four pulleys, as represented in Fig. 39. A single labourer would now be able to raise the load by the same exertion of force as each one of the four put forth. But when he

pulls the rope through four feet, the load only rises one foot, for the length through which he pulls the rope, at *a*, is uniformly distributed in the block over four ropes, so that each of these is only shortened by a foot. To raise the load, therefore, to the same height, the one man must necessarily work four times as long as the four together did. But the total expenditure of work is the same, whether four labourers work for a quarter of an hour or one works for an hour.

Fig. 39.

If, instead of human labour, we introduce the work of a weight, and hang to the block a load of 400, and at *a*, where otherwise the labourer works, a weight of 100 pounds, the block is then in equilibrium, and, without any appreciable exertion of the arm, may be set in motion. The weight of 100 pounds sinks, that of 400 rises. Without any measurable expenditure of force, the heavy weight has been raised by the sinking of the smaller one. But observe that the smaller weight will have sunk through four times the distance that the greater one has risen. But a fall of 100 pounds through four feet is just as much 400 foot pounds as a fall of 400 pounds through one foot.

The action of levers in all their various modifications

is precisely similar. Let $a\,b$, Fig. 40, be a simple lever, supported at c, the arm $c\,b$ being four times as long as the other arm $a\,c$. Let a weight of one pound be hung at b, and a weight of four pounds at a, the lever is then in equilibrium, and the least pressure of the finger is sufficient, without any appreciable exertion of force, to place it in the position $a'\,b'$, in which the heavy weight of four pounds has been raised, while the one-pound weight has sunk. But here, also, you will observe no work has been gained, for while the heavy weight has been raised

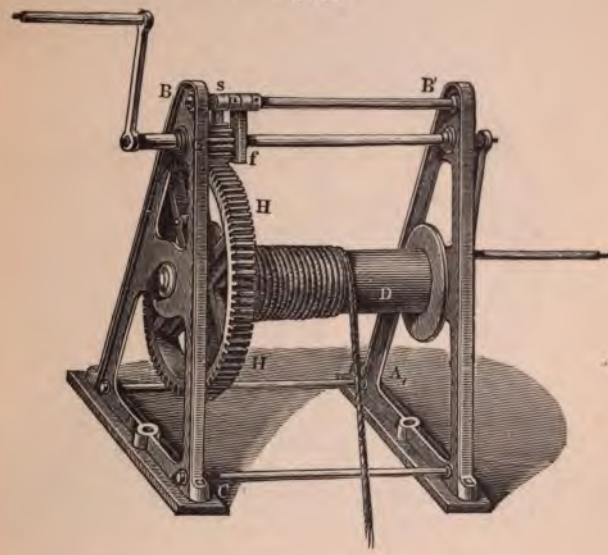
Fig. 40.

through one inch, the lighter one has fallen through four inches; and four pounds through one inch is, as work, equivalent to the product of one pound through four inches.

Most other fixed parts of machines may be regarded as modified and compound levers; a toothed-wheel, for instance as a series of levers, the ends of which are represented by the individual teeth, and one after the other of which is put in activity, in the degree in which the tooth in question seizes, or is seized by the adjacent pinion. Take, for instance, the crabwinch, represented in Fig. 41. Suppose the pinion on the axis of the barrel of

the winch has twelve teeth, and the toothed-wheel, H H, seventy-two teeth, that is six times as many as the former. The winch must now be turned round six times before the toothed-wheel, H, and the barrel, D, have made one turn, and before the rope which raises the load has been lifted by a length equal to the circumference of the barrel. The workman thus requires six times the

FIG. 41.

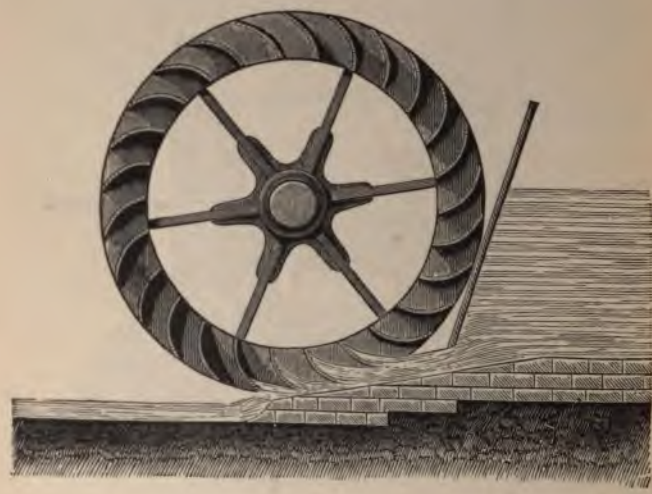


time, though to be sure only one-sixth of the exertion, which he would have to use if the handle were directly applied to the barrel, D. In all these machines, and parts of machines, we find it confirmed that in proportion as the velocity of the motion increases its power diminishes, and that when the power increases the velocity diminishes, but that the amount of work is never thereby increased.

In the overshot mill-wheel, described above, water acts by its weight. But there is another form of mill-wheels,

what is called the *undershot wheel*, in which it only acts by its impact, as represented in Fig. 42. These are used where the height from which the water comes is not great enough to flow on the upper part of the wheel. The lower part of undershot wheels dips in the flowing water which strikes against their float-boards and carries them along. Such wheels are used in swift-flowing streams which have a scarcely perceptible fall, as, for instance, on

FIG. 42.



the Rhine. In the immediate neighbourhood of such a wheel, the water need not necessarily have a great fall if it only strikes with considerable velocity. It is the velocity of the water, exerting an impact against the float-boards, which acts in this case, and which produces the motive power.

Windmills, which are used in the great plains of Holland and North Germany to supply the want of falling water, afford another instance of the action of velocity. The

sails are driven by air in motion—by wind. Air at rest could just as little drive a windmill as water at rest a water-wheel. The driving force depends here on the velocity of moving masses.

A bullet resting in the hand is the most harmless thing in the world; by its gravity it can exert no great effect; but when fired and endowed with great velocity it drives through all obstacles with the most tremendous force.

If I lay the head of a hammer gently on a nail, neither its small weight nor the pressure of my arm is quite sufficient to drive the nail into wood; but if I swing the hammer and allow it to fall with great velocity, it acquires a new force, which can overcome far greater hindrances.

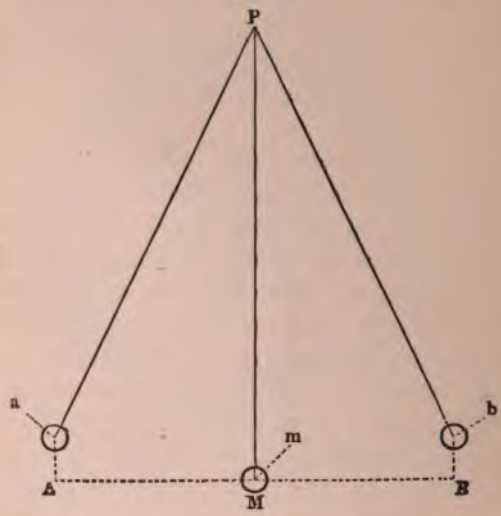
These examples teach us that the velocity of a moving mass can act as motive force. In mechanics, velocity in so far as it is motive force, and can produce work, is called *vis viva*. The name is not well chosen; it is too apt to suggest to us the force of living beings. Also in this case you will see, from the instances of the hammer and of the bullet, that velocity is lost as such, when it produces working power. In the case of the water-mill, or of the windmill, a more careful investigation of the moving masses of water and air is necessary to prove that part of their velocity has been lost by the work which they have performed.

The relation of velocity to working power is most simply and clearly seen in a simple pendulum, such as can be constructed by any weight which we suspend to a cord. Let M, Fig. 43, be such a weight, of a spherical form; A B, a horizontal line drawn through the centre of the sphere; P the point at which the cord is fastened. If now I draw the weight M on one side towards A, it moves in the arc M *a*, the end of which, *a*, is somewhat higher than the point A in the horizontal line. The weight is thereby

raised to the height A *a*. Hence my arm must exert a certain force to bring the weight to *a*. Gravity resists this motion and endeavours to bring back the weight to M, the lowest point which it can reach.

Now, if after I have brought the weight to *a* I let it go, it obeys this force of gravity and returns to M, arrives there with a certain velocity, and no longer remains quietly hanging at M as it did before, but swings be-

FIG. 43.



yond M towards *b*, where its motion stops as soon as it has traversed on the side of B an arc equal in length to that on the side of A, and after it has risen to a distance B *b* above the horizontal line, which is equal to the height A *a*, to which my arm had previously raised it. In *b* the pendulum returns, swings the same way back through M towards *a*, and so on, until its oscillations are gradually diminished, and ultimately annulled by the resistance of the air and by friction.

You see here that the reason why the weight, when it comes from *a* to M, and does not stop there, but ascends to *b*, in opposition to the action of gravity, is only to be sought in its velocity. The velocity which it has acquired in moving from the height A *a* is capable of again raising it to an equal height, B *b*. The velocity of the moving mass, M, is thus capable of raising this mass; that is to say, in the language of mechanics, of performing work. This would also be the case if we had imparted such a velocity to the suspended weight by a blow.

From this we learn further how to measure the working power of velocity—or, what is the same thing, the *vis viva* of the *moving mass*. It is equal to the work, expressed in foot pounds, which the same mass can exert after its velocity has been used to raise it, under the most favourable circumstances, to as great a height as possible.[1] This does not depend on the direction of the velocity; for if we swing a weight attached to a thread in a circle, we can even change a downward motion into an upward one.

The motion of the pendulum shows us very distinctly how the forms of working power hitherto considered—that of a raised weight and that of a moving mass—may merge into one another. In the points *a* and *b*, Fig. 43, the mass has no velocity; at the point M it has fallen as far as possible, but possesses velocity. As the weight goes from *a* to *m* the work of the raised weight is changed into *vis viva;* as the weight goes further from *m* to *b* the *vis viva* is changed into the work of a raised weight. Thus the work which the arm originally imparted to the pendulum is not lost in these oscillations, provided we may leave out of consideration the influence of the resistance of the air

[1] The measure of *vis viva* in theoretical mechanics is half the product of the weight into the square of the velocity. To reduce it to the technical measure of the work we must divide it by the intensity of gravity; that is, by the velocity at the end of the first second of a freely falling body.

and of friction. Neither does it increase, but it continually changes the form of its manifestation.

Let us now pass to other mechanical forces, those of elastic bodies. Instead of the weights which drive our clocks, we find in time-pieces and in watches, steel springs which are coiled in winding up the clock, and are uncoiled by the working of the clock. To coil up the spring we consume the force of the arm; this has to overcome the resisting elastic force of the spring as we wind it up, just as in the clock we have to overcome the force of gravity which the weight exerts. The coiled spring can, however, perform work; it gradually expends this acquired capability in driving the clockwork.

If I stretch a crossbow and afterwards let it go, the stretched string moves the arrow; it imparts to it force in the form of velocity. To stretch the cord my arm must work for a few seconds; this work is imparted to the arrow at the moment it is shot off. Thus the crossbow concentrates into an extremely short time the entire work which the arm had communicated in the operation of stretching; the clock, on the contrary, spreads it over one or several days. In both cases no work is produced which my arm did not originally impart to the instrument, it is only expended more conveniently.

The case is somewhat different if by any other natural process I can place an elastic body in a state of tension without having to exert my arm. This is possible and is most easily observed in the case of gases.

If, for instance, I discharge a fire-arm loaded with gunpowder, the greater part of the mass of the powder is converted into gases at a very high temperature, which have a powerful tendency to expand, and can only be retained in the narrow space in which they are formed, by the exercise of the most powerful pressure. In expanding with enormous force they propel the bullet,

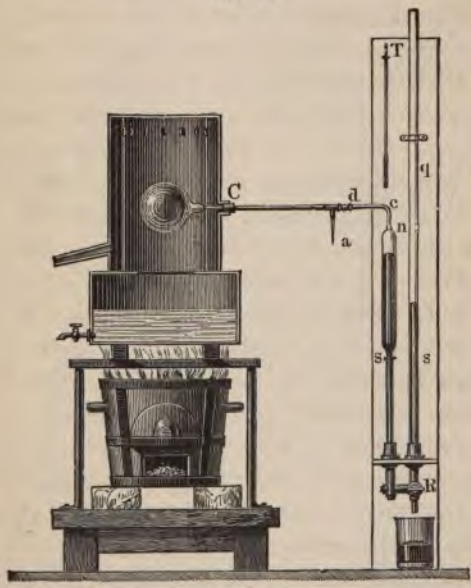
and impart to it a great velocity, which we have already seen is a form of work.

In this case, then, I have gained work which my arm has not performed. Something, however, has been lost; the gunpowder, that is to say, whose constituents have changed into other chemical compounds, from which they cannot, without further ado, be restored to their original condition. Here, then, a chemical change has taken place, under the influence of which work has been gained.

Elastic forces are produced in gases by the aid of heat, on a far greater scale.

Let us take, as the most simple instance, atmospheric air. In Fig. 44 an apparatus is represented such as

Fig. 44.



Regnault used for measuring the expansive force of heated gases. If no great accuracy is required in the measure-

ment, the apparatus may be arranged more simply. At C is a glass globe filled with dry air, which is placed in a metal vessel, in which it can be heated by steam. It is connected with the U-shaped tube, s s, which contains a liquid, and the limbs of which communicate with each other when the stop-cock R is closed. If the liquid is in equilibrium in the tube s s when the globe is cold, it rises in the leg s, and ultimately overflows when the globe is heated. If, on the contrary, when the globe is heated, equilibrium be restored by allowing some of the liquid to flow out at R, as the globe cools it will be drawn up towards *n*. In both cases liquid is raised, and work thereby produced.

The same experiment is continuously repeated on the largest scale in steam engines, though in order to keep up a continual disengagement of compressed gases from the boiler, the air in the globe in Fig. 44, which would soon reach the maximum of its expansion, is replaced by water, which is gradually changed into steam by the application of heat. But steam, so long as it remains as such, is an elastic gas which endeavours to expand exactly like atmospheric air. And instead of the column of liquid which was raised in our last experiment, the machine is caused to drive a solid piston which imparts its motion to other parts of the machine. Fig. 45 represents a front view of the working parts of a high pressure engine, and Fig. 46 a section. The boiler in which steam is generated is not represented; the steam passes through the tube z z, Fig. 46, to the cylinder A A, in which moves a tightly fitting piston C. The parts between the tube z z and the cylinder A A, that is the slide valve in the valve-chest K K, and the two tubes d and e allow the steam to pass first below and then above the piston, while at the same time the steam has free exit from the other half of the cylinder. When the

Fig. 45.

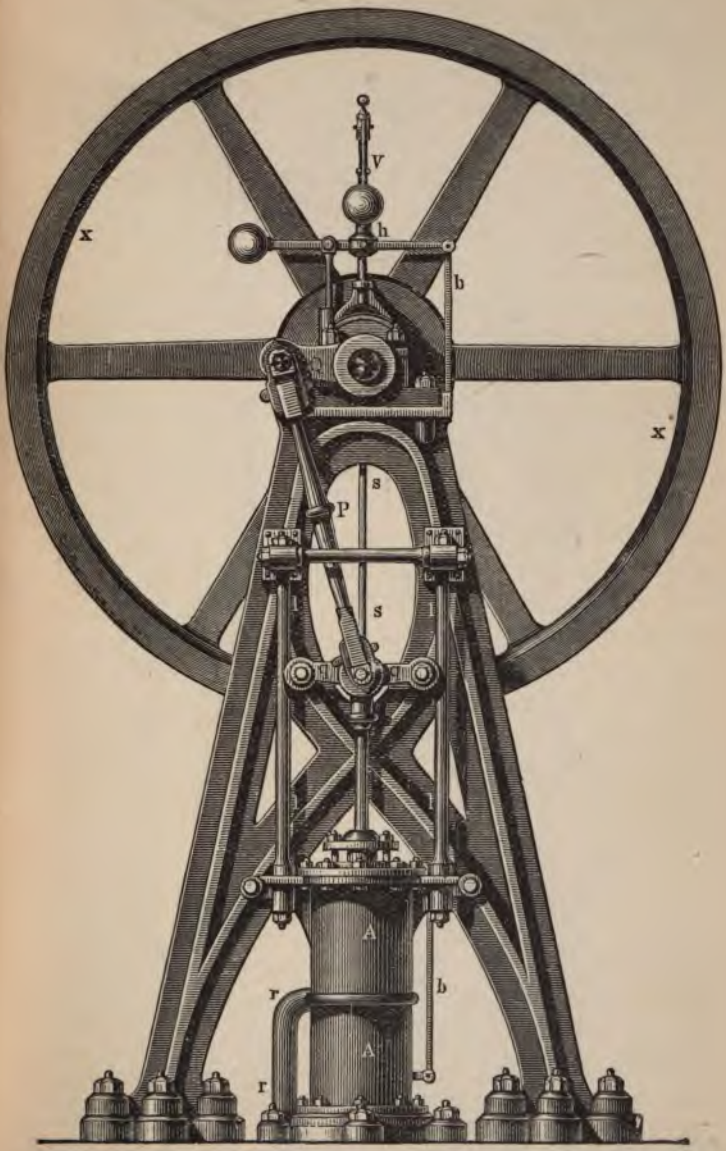
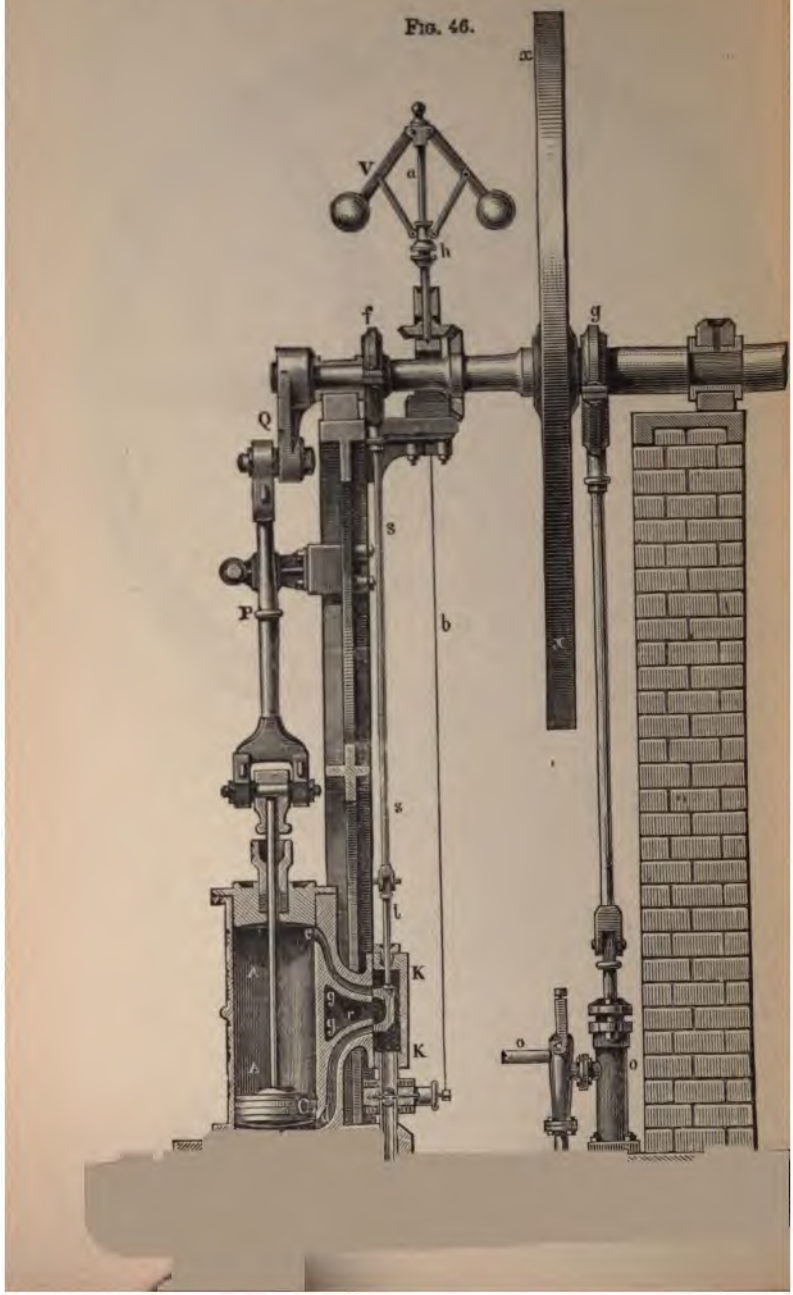


Fig. 46.



steam passes under the piston, it forces it upward; when the piston has reached the top of its course the position of the valve in K K changes, and the steam passes above the piston and forces it down again. The piston-rod acts by means of the connecting-rod P, on the crank Q of the fly-wheel X and sets this in motion. By means of the rod S, the motion of the rod regulates the opening and closing of the valve. But we need not here enter into those mechanical arrangements, however ingeniously they have been devised. We are only interested in the manner in which heat produces elastic vapour, and how this vapour, in its endeavour to expand, is compelled to move the solid parts of the machine, and furnish work.

You all know how powerful and varied are the effects of which steam engines are capable; with them has really begun the great development of industry which has characterised our century before all others. Its most essential superiority over motive powers formerly known, is that it is not restricted to a particular place. The store of coal and the small quantity of water which are the sources of its power can be brought everywhere, and steam engines can even be made movable, as is the case with steam-ships and locomotives. By means of these machines we can develope motive power to almost an indefinite extent at any place on the earth's surface, in deep mines and even on the middle of the ocean; while water and wind-mills are bound to special parts of the surface of the land. The locomotive transports travellers and goods over the land in numbers and with a speed which must have seemed an incredible fable to our forefathers, who looked upon the mail-coach with its six passengers in the inside and its ten miles an hour, as an enormous progress. Steam-engines traverse the ocean independently of the direction of the wind, and, successfully resisting storms which would drive

sailing-vessels far away, reach their goal at the appointed time. The advantages which the concourse of numerous, and variously skilled workmen in all branches offers in large towns where wind and water power are wanting, can be utilised, for steam-engines find place everywhere, and supply the necessary crude force; thus the more intelligent human force may be spared for better purposes; and, indeed, wherever the nature of the ground or the neighbourhood of suitable lines of communication present a favourable opportunity for the development of industry, the motive power is also present in the form of steam-engines.

We see, then, that heat can produce mechanical power; but in the cases which we have discussed we have seen that the quantity of force which can be produced by a given measure of a physical process is always accurately defined, and that the further capacity for work of the natural forces, is either diminished or exhausted by the work which has been performed. How is it now with *Heat* in this respect?

This question was of decisive importance in the endeavour to extend the law of the Conservation of Force to all natural processes. In the answer lay the chief difference between the older and newer views in these respects. Hence it is that many physicists designate that view of Nature corresponding to the law of the conservation of force with the name of *the Mechanical Theory of Heat.*

The older view of the nature of heat was that it is a substance, very fine and imponderable indeed, but indestructible, and unchangeable in quantity, which is an essential fundamental property of all matter. And, in fact, in a large number of natural processes, the quantity of heat which can be demonstrated by the thermometer is unchangeable.

By conduction and radiation, it can indeed pass from hotter to colder bodies; but the quantity of heat which the former lose can be shown by the thermometer to have reappeared in the latter. Many processes, too, were known, especially in the passage of bodies from the solid to the liquid and gaseous states, in which heat disappeared—at any rate, as regards the thermometer. But when the gaseous body was restored to the liquid, and the liquid to the solid state, exactly the same quantity of heat reappeared which formerly seemed to have been lost. Heat was said *to have become latent*. On this view, liquid water differed from solid ice in containing a certain quantity of heat bound, which, just because it was bound, could not pass to the thermometer, and therefore was not indicated by it. Aqueous vapour contains a far greater quantity of heat thus bound. But if the vapour be precipitated, and the liquid water restored to the state of ice, exactly the same amount of heat is liberated as had become latent in the melting of the ice and in the vaporisation of the water.

Finally, heat is sometimes produced and sometimes disappears in chemical processes. But even here it might be assumed that the various chemical elements and chemical compounds contain certain constant quantities of latent heat, which, when they change their composition, are sometimes liberated and sometimes must be supplied from external sources. Accurate experiments have shown that the quantity of heat which is developed by a chemical process, for instance, in burning a pound of pure carbon into carbonic acid, is perfectly constant, whether the combustion is slow or rapid, whether it takes place all at once or by intermediate stages. This also agreed very well with the assumption, which was the basis of the theory of heat, that heat is a substance entirely unchangeable in quantity. The natural processes

which have here been briefly mentioned, were the subject of extensive experimental and mathematical investigations, especially of the great French physicists in the last decade of the former, and the first decade of the present, century; and a rich and accurately-worked chapter of physics had been developed, in which everything agreed excellently with the hypothesis—that heat is a substance. On the other hand, the invariability in the quantity of heat in all these processes could at that time be explained in no other manner than that heat is a substance.

But one relation of heat—namely, that to mechanical work—had not been accurately investigated. A French engineer, Sadi Carnot, son of the celebrated War Minister of the Revolution, had indeed endeavoured to deduce the work which heat performs, by assuming that the hypothetical caloric endeavoured to expand like a gas; and from this assumption he deduced in fact a remarkable law as to the capacity of heat for work, which even now, though with an essential alteration introduced by Clausius, is among the bases of the modern mechanical theory of heat, and the practical conclusions from which, so far as they could at that time be compared with experiments, have held good.

But it was already known that whenever two bodies in motion rubbed against each other, heat was developed anew, and it could not be said whence it came.

The fact is universally recognised; the axle of a carriage which is badly greased and where the friction is great, becomes hot—so hot, indeed, that it may take fire; machine-wheels with iron axles going at a great rate may become so hot that they weld to their sockets. A powerful degree of friction is not, indeed, necessary to disengage an appreciable degree of heat; thus, a lucifer-match, which by rubbing is so heated that the phosphoric mass ignites, teaches this fact. Nay, it is enough to rub

the dry hands together to feel the heat produced by friction, and which is far greater than the heating which takes place when the hands lie gently on each other. Uncivilized people use the friction of two pieces of wood to kindle a fire. With this view, a sharp spindle of hard wood is made to revolve rapidly on a base of soft wood in the manner represented in Fig. 47.

Fig. 47.

So long as it was only a question of the friction of solids, in which particles from the surface become detached and compressed, it might be supposed that some changes in structure of the bodies rubbed might here liberate latent heat, which would thus appear as heat of friction.

But heat can also be produced by the friction of liquids, in which there could be no question of changes in structure, or of the liberation of latent heat. The first decisive experiment of this kind was made by Sir Humphry Davy in the commencement of the present century. In

a cooled space he made two pieces of ice rub against each other, and thereby caused them to melt. The latent heat which the newly formed water must have here assimilated could not have been conducted to it by the cold ice, or have been produced by a change of structure; it could have come from no other cause than from friction, and must have been created by friction.

Heat can also be produced by the impact of imperfectly elastic bodies as well as by friction. This is the case, for instance, when we produce fire by striking flint against steel, or when an iron bar is worked for some time by powerful blows of the hammer.

If we inquire into the mechanical effects of friction and of inelastic impact, we find at once that these are the processes by which all terrestrial movements are brought to rest. A moving body whose motion was not retarded by any resisting force would continue to move to all eternity. The motions of the planets are an instance of this. This is apparently never the case with the motion of the terrestrial bodies, for they are always in contact with other bodies which are at rest, and rub against them. We can, indeed, very much diminish their friction, but never completely annul it. A wheel which turns about a well-worked axle, once set in motion continues it for a long time; and the longer, the more truly and smoother the axle is made to turn, the better it is greased, and the less the pressure it has to support. Yet the *vis viva* of the motion which we have imparted to such a wheel when we started it, is gradually lost in consequence of friction. It disappears, and if we do not carefully consider the matter, it seems as if the *vis viva* which the wheel had possessed had been simply destroyed without any substitute.

A bullet which is rolled on a smooth horizontal surface continues to roll until its velocity is destroyed by fric-

tion on the path, caused by the very minute impacts on its little roughnesses.

A pendulum which has been put in vibration can continue to oscillate for hours if the suspension is good, without being driven by a weight; but by the friction against the surrounding air, and by that at its place of suspension, it ultimately comes to rest.

A stone which has fallen from a height has acquired a certain velocity on reaching the earth; this we know is the equivalent of a mechanical work; so long as this velocity continues as such, we can direct it upwards by means of suitable arrangements, and thus utilise it to raise the stone again. Ultimately the stone strikes against the earth and comes to rest; the impact has destroyed its velocity, and therewith apparently also the mechanical work which this velocity could have effected.

If we review the result of all these instances, which each of you could easily add to from your own daily experience, we shall see that friction and inelastic impact are processes in which mechanical work is destroyed, and heat produced in its place.

The experiments of Joule, which have been already mentioned, lead us a step further. He has measured in foot pounds the amount of work which is destroyed by the friction of solids and by the friction of liquids; and, on the other hand, he has determined the quantity of heat which is thereby produced, and has established a definite relation between the two. His experiments show that when heat is produced by the consumption of work, a definite quantity of work is required to produce that amount of heat which is known to physicists as the *unit of heat*; the heat, that is to say, which is necessary to raise one gramme of water through one degree centigrade. The quantity of work necessary for this is, according to Joule's best experiments, equal to the work which a

gramme would perform in falling through a height of 425 metres.

In order to show how closely concordant are his numbers, I will adduce the results of a few series of experiments which he obtained after introducing the latest improvements in his methods.

1. A series of experiments in which water was heated by friction in a brass vessel. In the interior of this vessel a vertical axle provided with sixteen paddles was rotated, the eddies thus produced being broken by a series of projecting barriers, in which parts were cut out large enough for the paddles to pass through. The value of the equivalent was 424·9 metres.

2. Two similar experiments, in which mercury in an iron vessel was substituted for water in a brass one, gave 425 and 426·3 metres.

3. Two series of experiments, in which a conical ring rubbed against another, both surrounded by mercury, gave 426·7 and 425·6 metres.

Exactly the same relations between heat and work were also found in the reverse process—that is, when work was produced by heat. In order to execute this process under physical conditions that could be controlled as perfectly as possible, permanent gases and not vapours were used, although the latter are, in practice, more convenient for producing large quantities of work, as in the case of the steam-engine. A gas which is allowed to expand with moderate velocity becomes cooled. Joule was the first to show the reason of this cooling. For the gas has, in expanding, to overcome the resistance, which the pressure of the atmosphere and the slowly yielding side of the vessel oppose to it; or, if it cannot of itself overcome this resistance, it supports the arm of the observer which does it. Gas thus performs work, and this work is produced at the cost of its heat. Hence the

cooling. If, on the contrary, the gas is suddenly allowed to issue into a perfectly exhausted space where it finds no resistance, it does not become cool as Joule has shown; or if individual parts of it become cool, others become warm; and, after the temperature has become equalised, this is exactly as much as before the sudden expansion of the gaseous mass.

How much heat the various gases disengage when they are compressed, and how much work is necessary for their compression; or, conversely, how much heat disappears when they expand under a pressure equal to their own counterpressure, and how much work they thereby effect in overcoming this counterpressure, was partly known from the older physical experiments, and has partly been determined by the recent experiments of Regnault by extremely perfect methods. Calculations with the best data of this kind give us the value of the thermal equivalent from experiments:—

With atmospheric air	. . . . .	426·0 metres.
„ oxygen	. . . . .	425·7 „
„ nitrogen	. . . . .	431·3 „
„ hydrogen	. . . . .	425·3 „

Comparing these numbers with those which determine the equivalence of heat and mechanical work in friction, as close an agreement is seen as can at all be expected from numbers which have been obtained by such varied investigations of different observers.

Thus then: a certain quantity of heat may be changed into a definite quantity of work; this quantity of work can also be retransformed into heat, and, indeed, into exactly the same quantity of heat as that from which it originated; in a mechanical point of view, they are exactly equivalent. Heat is a new form in which a quantity of work may appear.

These facts no longer permit us to regard heat as a

substance, for its quantity is not unchangeable. It can be produced anew from the *vis viva* of motion destroyed; it can be destroyed, and then produces motion. We must rather conclude from this that heat itself is a motion, an internal invisible motion of the smallest elementary particles of bodies. If, therefore, motion seems lost in friction and impact, it is not actually lost, but only passes from the great visible masses to their smallest particles; while in steam-engines the internal motion of the heated gaseous particles is transferred to the piston of the machine, accumulated in it, and combined in a resultant whole.

But what is the nature of this internal motion, can only be asserted with any degree of probability in the case of gases. Their particles probably cross one another in rectilinear paths in all directions, until, striking another particle, or against the side of the vessel, they are reflected in another direction. A gas would thus be analogous to a swarm of gnats, consisting, however, of particles infinitely small and infinitely more closely packed. This hypothesis, which has been developed by Krönig, Clausius, and Maxwell, very well accounts for all the phenomena of gases.

What appeared to the earlier physicists to be the constant quantity of heat is nothing more than the whole motive power of the motion of heat, which remains constant so long as it is not transformed into other forms of work, or results afresh from them.

We turn now to another kind of natural forces which can produce work—I mean the chemical. We have to-day already come across them. They are the ultimate cause of the work which gunpowder and the steam-engine produce; for the heat which is consumed in the latter, for example, originates in the combustion of carbon—that is to say, in a chemical process. The burning of

coal is the chemical union of carbon with the oxygen of the air, taking place under the influence of the chemical affinity of the two substances.

We may regard this force as an attractive force between the two, which, however, only acts through them with extraordinary power, if the smallest particles of the two substances are in closest proximity to each other. In combustion this force acts; the carbon and oxygen atoms strike against each other and adhere firmly, inasmuch as they form a new compound—carbonic acid—a gas known to all of you as that which ascends from all fermenting and fermented liquids—from beer and champagne. Now this attraction between the atoms of carbon and of oxygen performs work just as much as that which the earth in the form of gravity exerts upon a raised weight. When the weight falls to the ground, it produces an agitation, which is partly transmitted to the vicinity as sound waves, and partly remains as the motion of heat. The same result we must expect from chemical action. When carbon and oxygen atoms have rushed against each other, the newly-formed particles of carbonic acid must be in the most violent molecular motion—that is, in the motion of heat. And this is so. A pound of carbon burned with oxygen to form carbonic acid, gives as much heat as is necessary to raise 80·9 pounds of water from the freezing to the boiling point; and just as the same amount of work is produced when a weight falls, whether it falls slowly or fast, so also the same quantity of heat is produced by the combustion of carbon, whether this is slow or rapid, whether it takes place all at once, or by successive stages

When the carbon is burned, we obtain in its stead, and in that of the oxygen, the gaseous product of combustion —carbonic acid. Immediately after combustion it is incandescent. When it has afterwards imparted heat to the vicinity, we have in the carbonic acid the entire quantity

of carbon and the entire quantity of oxygen, and also the force of affinity quite as strong as before. But the action of the latter is now limited to holding the atoms of carbon and oxygen firmly united; they can no longer produce either heat or work any more than a fallen weight can do work if it has not been again raised by some extraneous force. When the carbon has been burnt we take no further trouble to retain the carbonic acid; it can do no more service, we endeavour to get it out of the chimneys of our houses as fast as we can.

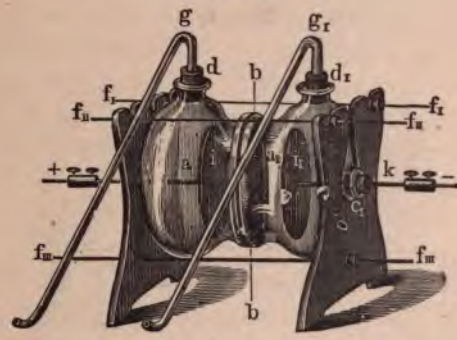
Is it possible, then, to tear asunder the particles of carbonic acid, and give to them once more the capacity of work which they had before they were combined, just as we can restore the potentiality of a weight by raising it from the ground? It is indeed possible. We shall afterwards see how it occurs in the life of plants; it can also be effected by inorganic processes, though in roundabout ways, the explanation of which would lead us too far from our present course.

This can, however, be easily and directly shown for another element, hydrogen, which can be burnt just like carbon. Hydrogen with carbon is a constituent of all combustible vegetable substances, among others, it is also an essential constituent of the gas which is used for lighting our streets and rooms; in the free state it is also a gas, the lightest of all, and burns when ignited with a feebly luminous blue flame. In this combustion—that is, in the chemical combination of hydrogen with oxygen, a very considerable quantity of heat is produced; for a given weight of hydrogen, four times as much heat as in the combustion of the same weight of carbon. The product of combustion is water, which, therefore, is not of itself further combustible, for the hydrogen in it is completely saturated with oxygen. The force of affinity,

therefore, of hydrogen for oxygen, like that of carbon for oxygen, performs work in combustion, which appears in the form of heat. In the water which has been formed during combustion, the force of affinity is exerted between the elements as before, but its capacity for work is lost. Hence the two elements must be again separated, their atoms torn apart, if new effects are to be produced from them.

This we can do by the aid of currents of electricity. In the apparatus depicted in Fig. 48, we have two glass

FIG. 48.



vessels filled with acidulated water, a and a¹, which are separated in the middle by a porous plate moistened with water. In both sides are fitted platinum wires, k, which are attached to platinum plates, i and i¹. As soon as a galvanic current is transmitted through the water by the platinum wires, k, you see bubbles of gas ascend from the plates i and i¹. These bubbles are the two elements of water, hydrogen on the one hand, and oxygen on the other. The gases emerge through the tubes g and g¹. If we wait until the upper part of the vessels and the tubes have been filled with it, we can inflame hydrogen at one side; it burns with a blue flame. If I bring a glimmering spill near the mouth of the other tube it

bursts into flame, just as happens with oxygen gas, in which the processes of combustion are far more intense than in atmospheric air, where the oxygen mixed with nitrogen is only one-fifth of the whole volume.

If I hold a glass flask filled with water over the hydrogen flame, the water, newly formed in combustion, condenses upon it.

If a platinum wire be held in the almost non-luminous flame, you see how intensely it is ignited; in a plentiful current of a mixture of the gases, hydrogen and oxygen, which have been liberated in the above experiment, the

Fig. 49.

almost infusible platinum might even be melted. The hydrogen which has here been liberated from the water by the electrical current has regained the capacity of producing large quantities of heat by a fresh combination with oxygen; its affinity for oxygen has regained for it its capacity for work.

We here become acquainted with a new source of work, the electric current which decomposes water. This current is itself produced by a galvanic battery, Fig. 49.

Each of the four vessels contains nitric acid, in which there is a hollow cylinder of very compact carbon. In the middle of the carbon cylinder is a cylindrical porous vessel of white clay, which contains dilute sulphuric acid; in this dips a zinc cylinder. Each zinc cylinder is connected by a metal ring with the carbon cylinder of the next vessel, the last zinc cylinder n is connected with one platinum plate, and the first carbon cylinder, p, with the other platinum plate of the apparatus for the decomposition of water.

If now the conducting circuit of this galvanic apparatus is completed, and the decomposition of water begins, a chemical process takes place simultaneously in the cells of the voltaic battery. Zinc takes oxygen from the surrounding water and undergoes a slow combustion. The product of combustion thereby produced, oxide of zinc, unites further with sulphuric acid, for which it has a powerful affinity, and sulphate of zinc, a saline kind of substance, dissolves in the liquid. The oxygen, moreover, which is withdrawn from it is taken by the water from the nitric acid surrounding the cylinder of carbon, which is very rich in it, and readily gives it up. Thus, in the galvanic battery zinc burns to sulphate of zinc at the cost of the oxygen of nitric acid.

Thus, while one product of combustion, water, is again separated, a new combustion is taking place—that of zinc. While we there reproduce chemical affinity which is capable of work, it is here lost. The electrical current is, as it were, only the carrier which transfers the chemical force of the zinc uniting with oxygen and acid to water in the decomposing cell, and uses it for overcoming the chemical force of hydrogen and oxygen.

In this case, we can restore work which has been lost, but only by using another force, that of oxidising zinc.

Here we have overcome chemical forces by chemical

forces, through the instrumentality of the electrical current. But we can attain the same object by mechanical

Fig. 50.

forces, if we produce the electrical current by a magneto-electrical machine, Fig. 50. If we turn the handle, the anker R R', on which is coiled copper-wire, rotates in front

of the poles of the horse-shoe magnet, and in these coils electrical currents are produced, which can be led from the points a and b. If the ends of these wires are connected with the apparatus for decomposing water we obtain hydrogen and oxygen, though in far smaller quantity than by the aid of the battery which we used before. But this process is interesting, for the mechanical force of the arm which turns the wheel produces the work which is required for separating the combined chemical elements. Just as the steam-engine changes chemical into mechanical force, the magneto-electrical machine transforms mechanical force into chemical.

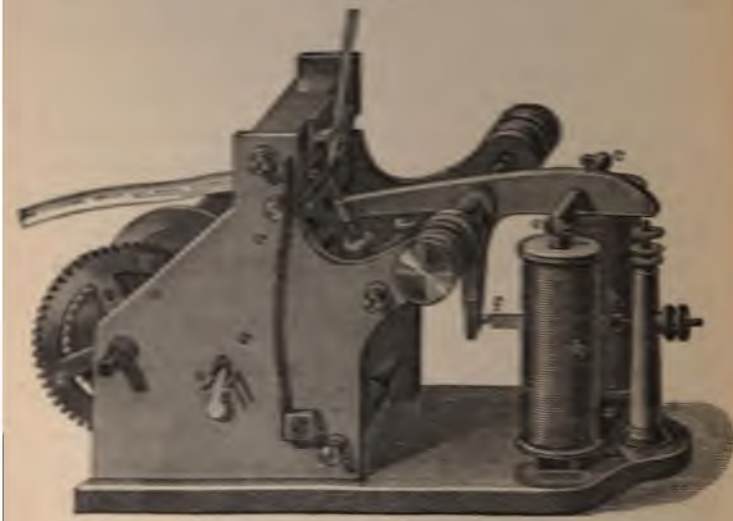
The application of electrical currents opens out a large number of relations between the various natural forces. We have decomposed water into its elements by such currents, and should be able to decompose a large number of other chemical compounds. On the other hand, in ordinary galvanic batteries electrical currents are produced by chemical forces.

In all conductors through which electrical currents pass they produce heat; I stretch a thin platinum wire between the ends n and p of the galvanic battery, Fig. 49; it becomes ignited and melts. On the other hand, electrical currents are produced by heat in what are called thermo-electric elements.

Iron which is brought near a spiral of copper wire, traversed by an electrical current, becomes magnetic, and then attracts other pieces of iron, or a suitably placed steel magnet. We thus obtain mechanical actions which meet with extended applications in the electrical telegraph, for instance. Fig. 51 represents a Morse's telegraph in one-third of the natural size. The essential part is a horse-shoe shaped iron core, which stands in the copper spirals b b. Just over the top of this is a small steel magnet c c, which is attracted the moment an

electrical current, arriving by the telegraph wire, traverses the spirals b b. The magnet e e is rigidly fixed in the lever d d, at the other end of which is a style; this makes a mark on a paper band, drawn by a clock-work, as often and as long as e e is attracted by the magnetic action of the electrical current. Conversely, by reversing the magnetism in the iron core of the spirals b b, we should obtain in them an electrical current just as we

FIG. 51.



have obtained such currents in the magneto-electrical machine, Fig. 50; in the spirals of that machine there is an iron core which, by being approached to the poles of the large horse-shoe magnet, is sometimes magnetised in one and sometimes in the other direction.

I will not accumulate examples of such relations; in subsequent lectures we shall come across them. Let us review these examples once more, and recognise in them the law which is common to all.

A raised weight can produce work, but in doing so it must necessarily sink from its height, and, when it has fallen as deep as it can fall, its gravity remains as before, but it can no longer do work.

A stretched spring can do work, but in so doing it becomes loose. The velocity of a moving mass can do work, but in doing so it comes to rest. Heat can perform work; it is destroyed in the operation. Chemical forces can perform work, but they exhaust themselves in the effort.

Electrical currents can perform work, but to keep them up we must consume either chemical or mechanical forces, or heat.

We may express this generally. *It is a universal character of all known natural forces that their capacity for work is exhausted in the degree in which they actually perform work.*

We have seen, further, that when a weight fell without performing any work, it *either* acquired velocity or produced heat. We might also drive a magneto-electrical machine by a falling weight; it would then furnish electrical currents.

We have seen that chemical forces, when they come into play, produce either heat or electrical currents or mechanical work.

We have seen that heat may be changed into work; there are apparatus (thermo-electric batteries) in which electrical currents are produced by it. Heat can directly separate chemical compounds; thus, when we burn limestone, it separates carbonic acid from lime.

Thus, whenever the capacity for work of one natural force is destroyed, it is transformed into another kind of activity. Even within the circuit of inorganic natural forces, we can transform each of them into an active condition by the aid of any other natural force which is

capable of work. The connections between the various natural forces which modern physics has revealed, are so extraordinarily numerous that several entirely different methods may be discovered for each of these problems.

I have stated how we are accustomed to measure mechanical work, and how the equivalent in work of heat may be found. The equivalent in work of chemical processes is again measured by the heat which they produce. By similar relations, the equivalent in work of the other natural forces may be expressed in terms of mechanical work.

If, now, a certain quantity of mechanical work is lost, there is obtained, as experiments made with the object of determining this point show, an equivalent quantity of heat, or, instead of this, of chemical force; and, conversely, when heat is lost, we gain an equivalent quantity of chemical or mechanical force; and, again, when chemical force disappears, an equivalent of heat or work; so that in all these interchanges between various inorganic natural forces working force may indeed disappear in one form, but then it reappears in exactly equivalent quantity in some other form; it is thus neither increased nor diminished, but always remains in exactly the same quantity. We shall subsequently see that the same law holds good also for processes in organic nature, so far as the facts have been tested.

It follows thence *that the total quantity of all the forces* capable of work *in the whole universe remains eternal and unchanged throughout all their changes.* All change in nature amounts to this, that force can change its form and locality without its quantity being changed. The universe possesses, once for all, a store of force which is not altered by any change of phenomena, can neither be increased nor diminished, and which maintains any change which takes place on it.

You see how, starting from considerations based on the immediate practical interests of technical work, we have been led up to a universal natural law, which, as far as all previous experience extends, rules and embraces all natural processes; which is no longer restricted to the practical objects of human utility, but expresses a perfectly general and particularly characteristic property of all natural forces, and which, as regards generality, is to be placed by the side of the laws of the unalterability of mass, and the unalterability of the chemical elements.

At the same time, it also decides a great practical question which has been much discussed in the last two centuries, to the decision of which an infinity of experiments have been made and an infinity of apparatus constructed—that is, the question of the possibility of a perpetual motion. By this was understood a machine which was to work continuously without the aid of any external driving force. The solution of this problem promised enormous gains. Such a machine would have had all the advantages of steam without requiring the expenditure of fuel. Work is wealth. A machine which could produce work from nothing was as good as one which made gold. This problem had thus for a long time occupied the place of gold making, and had confused many a pondering brain. That a perpetual motion could not be produced by the aid of the then known mechanical forces could be demonstrated in the last century by the aid of the mathematical mechanics which had at that time been developed. But to show also that it is not possible even if heat, chemical forces, electricity, and magnetism were made to co-operate, could not be done without a knowledge of our law in all its generality. The possibility of a perpetual motion was first finally negatived by the law of the conservation of force, and this law might also be ex-

pressed in the practical form that no perpetual motion is possible, that force cannot be produced from nothing; something must be consumed.

You will only be ultimately able to estimate the importance and the scope of our law when you have before your eyes a series of its applications to individual processes on nature.

What I have to-day mentioned as to the origin of the moving forces which are at our disposal, directs us to something beyond the narrow confines of our laboratories and our manufactories, to the great operations at work in the life of the earth and of the universe. The force of falling water can only flow down from the hills when rain and snow bring it to them. To furnish these, we must have aqueous vapour in the atmosphere, which can only be effected by the aid of heat, and this heat comes from the sun. The steam-engine needs the fuel which the vegetable life yields, whether it be the still active life of the surrounding vegetation, or the extinct life which has produced the immense coal deposits in the depths of the earth. The forces of man and animals must be restored by nourishment; all nourishment comes ultimately from the vegetable kingdom, and leads us back to the same source.

You see then that when we inquire into the origin of the moving forces which we take into our service, we are thrown back upon the meteorological processes in the earth's atmosphere, on the life of plants in general, and on the sun.

THE AIM AND PROGRESS OF PHYSICAL SCIENCE.

AN OPENING ADDRESS DELIVERED AT THE NATURFORSCHER VERSAMMLUNG, IN INNSBRÜCK, 1869.

In accepting the honour you have done me in requesting me to deliver the first lecture at the opening meeting of this year's Association, it appears to me to be more in keeping with the import of the moment and the dignity of this assembly that, in place of dealing with any particular line of research of my own, I should invite you to cast a glance at the development of all the branches of physical science represented on these occasions. These branches include a vast area of special investigation, material of almost too varied a character for comprehension, the range and intrinsic value of which become greater with each year, while no bounds can be assigned to its increase. During the first half of the present century we had an Alexander von Humboldt, who was able to scan the scientific knowledge of his time in its details, and to bring it within one vast generalisation. At the present juncture, it is obviously very doubtful whether this task could be accomplished in a similar way, even by a mind with gifts so peculiarly suited for the purpose as Humboldt's was, and if all his time and work were devoted to the purpose.

We, however, working as we do to advance a single

department of science, can devote but little of our time to the simultaneous study of the other branches. As soon as we enter upon any investigation, all our powers have to be concentrated on a field of narrowed limit. We have not only, like the philologian or historian, to seek out and search through books and gather from them what others have already determined about the subject under inquiry; that is but a secondary portion of our work. We have to attack the things themselves, and in doing so each offers new and peculiar difficulties of a kind quite different from those the scholar encounters; while in the majority of instances, most of our time and labour is consumed by secondary matters that are but remotely connected with the purpose of the investigation.

At one time, we have to study the errors of our instruments, with a view to their diminution, or, where they cannot be removed, to compass their detrimental influence; while at other times we have to watch for the moment when an organism presents itself under circumstances most favourable for research. Again, in the course of our investigation we learn for the first time of possible errors which vitiate the result, or perhaps merely raise a suspicion that it may be vitiated, and we find ourselves compelled to begin the work anew, till every shadow of doubt is removed. And it is only when the observer takes such a grip of the subject, so fixes all his thoughts and all his interest upon it that he cannot separate himself from it for weeks, for months, even for years, cannot force himself away from it, in short, till he has mastered every detail, and feels assured of all those results which must come in time, that a perfect and valuable piece of work is done. You are all aware that in every good research, the preparation, the secondary operations, the control of possible errors, and especially in the separation of the results attainable in the time from those that cannot

be attained, consume far more time than is really required to make actual observations or experiments. How much more ingenuity and thought are expended in bringing a refractory piece of brass or glass into subjection, than in sketching out the plan of the whole investigation! Each of you will have experienced such impatience and over-excitement during work where all the thoughts are directed on a narrow range of questions, the import of which to an outsider appears trifling and contemptible because he does not see the end to which the preparatory work tends. I believe I am correct in thus describing the work and mental condition that precedes all those great results which hastened so much the development of science after its long inaction, and gave it so powerful an influence over every phase of human life.

The period of work, then, is no time for broad comprehensive survey. When, however, the victory over difficulties has happily been gained, and results are secured, a period of repose follows, and our interest is next directed to examining the bearing of the newly established facts, and once more venturing on a wider survey of the adjoining territory. This is essential, and those only who are capable of viewing it in this light can hope to find useful starting-points for further investigation.

The preliminary work is followed by other work, treating of other subjects. In the course of its different stages, the observer will not deviate far from a direction of more or less narrowed range. For it is not alone of importance to him that he may have collected information from books regarding the region to be explored. The human memory is, on the whole, proportionately patient, and can store up an almost incredibly large amount of learning. In addition, however, to the knowledge which

the student of science acquires from lectures and books, he requires intelligence which only an ample and diligent perception can give him; he needs skill which comes only by repeated experiment and long practice. His senses must be sharpened for certain kinds of observation, to detect minute differences of form, colour, solidity, smell, &c., in the object under examination; his hand must be equally trained to the work of the blacksmith, the locksmith, and the carpenter, or the draughtsman and the violin-player, and, when operating with the microscope, must surpass the lace-maker in delicacy of handling the needle. Moreover, when he encounters superior destructive forces, or performs bloody operations upon man or beast, he must possess the courage and coolness of the soldier. Such qualities and capabilities, partly the result of natural aptitude, partly cultivated by long practice, are not so readily and so easily acquired as the mere massing of facts in the memory; and hence it happens that an investigator is compelled, during the entire labours of his life, to strictly limit his field, and to confine himself to those branches which suit him best.

We must not, however, forget that the more the individual worker is compelled to narrow the sphere of his activity, so much the more will his intellectual desires induce him not to sever his connection with the subject in its entirety. How shall he go stout and cheerful to his toilsome work, how feel confident that what has given him so much labour will not moulder uselessly away, but remain a thing of lasting value, unless he keeps alive within himself the conviction that he also has added a fragment to the stupendous whole of Science which is to make the reasonless forces of nature subservient to the moral purposes of humanity?

An immediate practical use cannot generally be counted on *à priori* for each particular investigation. Physical

science, it is true, has by the practical realisation of its results transformed the entire life of modern humanity. But, as a rule, these applications appear under circumstances when they are least expected; to search in that direction generally leads to nothing unless certain points have already been definitely fixed, so that all that has to be done is to remove certain obstacles in the way of practical application. If we search the records of the most important discoveries, they are either, especially in earlier times, made by workmen who their whole lives through did but one kind of work, and, either by a happy accident, or by a searching, repeated, tentative experiment, hit upon some new method advantageous to their particular handicraft; others there are, and this is especially the case in most of the recent discoveries, which are the fruit of a matured scientific acquaintance with the subject in question, an acquaintance that in each instance had originally been acquired without any direct view to possible use.

Our Association represents the whole of natural science. To-day are assembled mathematicians, physicists, chemists and zoologists, botanists and geologists, the teacher of science and the physician, the technologist and the amateur who finds in scientific pursuits relaxation from other occupation. Here each of us hopes to meet with fresh impulse and encouragement for his peculiar work; the man who lives in a small country place hopes to meet with the recognition, otherwise unattainable, of having aided in the advance of science; he hopes by intercourse with men pursuing more or less the same object to mark the aim of new researches. We rejoice to find among us a goodly proportion of members representing the cultivated classes of the nation; we see influential statesmen among us. They all have an interest in our labours; they look to us for further progress in civilisation, further

victories over the powers of nature. They it is who place at our disposal the actual means for carrying on our labours, and are therefore entitled to enquire into the results of those labours. It appears to me, therefore, appropriate to this occasion to take account of the progress of science as a whole, of the objects it aspires to, and the magnitude of the efforts made to attain them.

Such a survey is desirable; that it lies beyond the powers of any one man to accomplish with even an approximate completeness such a task as this is clear from what I have already said. If I stand here to-day with such a problem entrusted to me, my excuse must be that no other would attempt it, and I hold that an attempt to accomplish it, even if with small success, is better than none whatever. Besides, a physiologist has perhaps more than all others immediate occasion to maintain a clear and constant view of the entire field, for in the present state of things it is peculiarly the lot of the physiologist to receive help from all other branches of science and to stand in alliance with them. In physiology, in fact, the importance of the vast strides to which I shall allude, has been chiefly felt, while to physiology, and the leading controversies arising in it, some of the most valuable discoveries are directly due.

If I leave considerable gaps in my survey, my excuse must be the magnitude of the task, and the fact that the pressing summons of my friend the secretary of this Association reached me but recently, and that too in the course of my summer holiday in the mountains. The gaps which I may leave will at all events be abundantly filled up by the proceedings of the Sections.

Let us then proceed to our task. In discussing the progress of physical science as a whole, the first question which presents itself is, By what standard are we to estimate this progress?

To the uninitiated, this science of ours is an accumulation of a vast number of facts, some of which are conspicuous for their practical utility, while others are merely curiosities, or objects of wonder. And, if it were possible to classify this unconnected mass of facts, as was done in the Linnean system, or in encyclopædias, so that each may be readily found when required, such knowledge as this would not deserve the name of science, nor satisfy either the scientific wants of the human mind, or the desire for progressive mastery over the powers of nature. For the former requires an intellectual grasp of the connection of ideas, the latter demands our anticipation of a result in cases yet untried, and under conditions that we propose to introduce in the course of our experiment. Both are obviously arrived at by a knowledge of the *law* of the phenomena.

Isolated facts and experiments have in themselves no value, however great their number may be. They only become valuable in a theoretical or practical point of view when they make us acquainted with the *law* of a series of uniformly recurring phenomena, or, it may be, only give a negative result showing an incompleteness in our knowledge of such a law, till then held to be perfect. From the exact and universal conformity to law of natural phenomena, a single observation of a condition that we may presume to be rigorously conformable to law, suffices, it is true, at times to establish a rule with the highest degree of probability; just as, for example, we assume our knowledge of the skeleton of a prehistoric animal to be complete if we find only one complete skeleton of a single individual. But we must not lose sight of the fact that the isolated observation is not of value in that it is isolated, but because it is an aid to the knowledge of the conformable regularity in bodily structure of an entire species of organisms. In like manner, the knowledge of

the specific heat of one small fragment of a new metal is important because we have no grounds for doubting that any other pieces of the same metal subjected to the same treatment will yield the same result.

To find the *law* by which they are regulated is to *understand* phenomena. For law is nothing more than the general conception in which a series of similarly recurring natural processes may be embraced. Just as we include in the conception 'mammal' all that is common to the man, the ape, the dog, the lion, the hare, the horse, the whale, &c., so we comprehend in the law of refraction that which we observe to regularly recur when a ray of light of any colour passes in any direction through the common boundary of any two transparent media.

A law of nature, however, is not a mere logical conception that we have adopted as a kind of *memoria technica* to enable us to more readily remember facts. We of the present day have already sufficient insight to know that the laws of nature are not things which we can evolve by any speculative method. On the contrary, we have to *discover* them in the facts; we have to test them by repeated observation or experiment, in constantly new cases, under ever-varying circumstances; and in proportion only as they hold good under a constantly increasing change of conditions, in a constantly increasing number of cases and with greater delicacy in the means of observation, does our confidence in their trustworthiness rise.

Thus the laws of nature occupy the position of a power with which we are not familiar, not to be arbitrarily selected and determined in our minds, as one might devise various systems of animals and plants one after another, so long as the object is only one of classification. Before we can say that our knowledge of any one law of nature is complete, we must see that *it holds good*

without exception, and make this the test of its correctness. If we can be assured that the conditions under which the law operates have presented themselves, the result must ensue without arbitrariness, without choice, without our co-operation, and from the very necessity which regulates the things of the external world as well as our perception. The law then takes the form of an objective power, and for that reason we call it *force*.

For instance, we regard the law of refraction objectively as a refractive force in transparent substances; the law of chemical affinity as the elective force exhibited by different bodies towards one another. In the same way, we speak of electrical force of contact of metals, of a force of adhesion, capillary force, and so on. Under these names are stated objectively laws which for the most part comprise small series of natural processes, the conditions of which are somewhat involved. In science our conceptions begin in this way, proceeding to generalizations from a number of well-established special laws. We must endeavour to eliminate the incidents of form and distribution in space which masses under investigation may present by trying to find from the phenomena attending large visible masses laws for the operation of infinitely small particles; or, expressed objectively, by resolving the forces of composite masses into the forces of their smallest elementary particles. But precisely in this, the simplest form of expression of force—namely, of mechanical force acting on a point of the mass—is it especially clear that force is only the law of action objectively expressed. The force arising from the presence of such and such bodies is equivalent to the acceleration of the mass on which it operates multiplied by this mass. The actual meaning of such an equation is that it expresses the following law: if such and such masses are present and no other, such and such acceleration of their

individual points occurs. Its actual signification may be compared with the facts and tested by them. The abstract conception of force we thus introduce implies moreover, that we did not discover this law at random, that it is an essential law of phenomena.

Our desire to *comprehend* natural phenomena, in other words, to ascertain their *laws*, thus takes another form of expression—that is, we have to seek out the *forces* which are the *causes* of the phenomena. The conformity to law in nature must be conceived as a causal connection the moment we recognise that it is independent of our thought and will.

If then we direct our inquiry to the progress of physical science as a whole, we shall have to judge of it by the measure in which the recognition and knowledge of a causative connection embracing all natural phenomena has advanced.

On looking back over the history of our sciences, the first great example we find of the subjugation of a wide mass of facts to a comprehensive law, occurred in the case of theoretical mechanics, the fundamental conception of which was first clearly propounded by Galileo. The question then was to find the general propositions that to us now appear so self-evident, that all substance is inert, and that the magnitude of force is to be measured not by its velocity, but by changes in it. At first the operation of a continually acting force could only be represented as a series of small impacts. It was not till Leibnitz and Newton, by the discovery of the differential calculus, had dispelled the ancient darkness which enveloped the conception of the infinite, and had clearly established the conception of the Continuous and of continuous change, that a full and productive application of the newly-found mechanical conceptions made any progress. The most singular and most splendid instance of such an applica-

tion was in regard to the motion of the planets, and I need scarcely remind you here how brilliant an example astronomy has been for the development of the other branches of science. In its case, by the theory of gravitation, a vast and complex mass of facts were first embraced in a single principle of great simplicity, and such a reconciliation of theory and fact established as has never been accomplished in any other department of science, either before or since. In supplying the wants of astronomy, have originated almost all the exact methods of measurement as well as the principal advances made in modern mathematics; the science itself was peculiarly fitted to attract the attention of the general public, partly by the grandeur of the objects under investigation, partly by its practical utility in navigation and geodesy, and the many industrial and social interests arising from them.

Galileo began with the study of terrestrial gravity. Newton extended the application, at first cautiously and hesitatingly, to the moon, then boldly to all the planets. And, in more recent times, we learn that these laws of the common inertia and gravitation of all ponderable masses hold good of the movements of the most distant double stars of which the light has yet reached us.

During the latter half of the last and the first half of the present century came the great progress of chemistry which conclusively solved the ancient problem of discovering the elementary substances, a task to which so much metaphysical speculation had been devoted. Reality has always far exceeded even the boldest and wildest speculation, and, in the place of the four primitive metaphysical elements—fire, water, air, and earth—we have now the sixty-five simple bodies of modern chemistry. Science has shown that these elements are really indestructible, unalterable in their mass, unalterable also in their properties;

in short, that from every condition into which they may have been converted, they can invariably be isolated, and recover those qualities which they previously possessed in the free state. Through all the varied phases of the phenomena of animated and inanimate nature, so far as we are acquainted with them, in all the astonishing results of chemical decomposition and combination, the number and diversity of which the chemist with unwearied diligence augments from year to year, the one law of the *immutability of matter* prevails as a necessity that knows no exception. And chemistry has already pressed on into the depths of immeasurable space, and detected in the most distant suns or nebulæ indications of well-known terrestrial elements, so that doubts respecting the prevailing homogeneity of the matter of the universe no longer exist, though certain elements may perhaps be restricted to certain groups of the heavenly bodies.

From this invariability of the elements follows another and wider consequence. Chemistry shows by actual experiment that all matter is made up of the elements which have been already isolated. These elements may exhibit great differences as regards combination or mixture, the mode of aggregation or molecular structure—that is to say, they may vary the mode of their *distribution in space*. In their *properties*, on the other hand, they are altogether unchangeable; in other words, when referred to the same compound, as regards isolation, and to the same state of aggregation, they invariably exhibit the same properties as before. If, then, all elementary substances are unchangeable in respect to their properties, and only changeable as regards their combination and their states of aggregation—that is, in respect to their distribution in space—it follows that all changes in the world are changes in the local distribution of elementary matter, and are eventually brought about through *Motion*.

If, however, motion be the primordial change which lies at the root of all the other changes occurring in the world, every elementary force is a force of motion, and the ultimate aim of physical science must be to determine the movements which are the real causes of all other phenomena and discover the motive powers on which they depend; in other words, to merge itself into mechanics.

Though this is clearly the final consequence of the qualitative and quantitative immutability of matter, it is after all an ideal proposition, the realization of which is still very remote. The field is a prescribed one, in which we have succeeded in tracing back actually observed changes to motions and forces of motion of a definite kind. Besides astronomy, may be mentioned the purely mechanical part of physics, then acoustics, optics, and electricity; in the science of heat and in chemistry, strenuous endeavours are being made towards perfecting definite views respecting the nature of the motion and position of molecules, while physiology has scarcely made a definite step in this direction.

This renders all the more important, therefore, a noteworthy advancement of the most general importance made during the last quarter of a century in the direction we are considering. If all elementary forces are forces of motion, and all, consequently, of similar nature, they should all be measurable by the same standard, that is, the standard of the mechanical forces. And that this is actually the fact is now regarded as proved. The law expressing this is known under the name of *the law of the Conservation of Force.*

For a small group of natural phenomena it had already been pronounced by Newton, then more definitely and in more general terms by D. Bernouilli, and so continued of recognised application in the greater part of the then known purely mechanical processes. Certain amplifica-

tions at times attracted attention, like those of Rumford, Davy, and Montgolfier. The first, however, to compass the clear and distinct idea of this law, and to venture to pronounce its absolute universality, was one whom we shall have soon the pleasure of hearing from this platform, Dr. Robert Mayer, of Heilbronn. While Dr. Mayer was led by physiological questions to the discovery of the most general form of this law, technical questions in mechanical engineering led Mr. Joule, of Manchester, simultaneously, and independently of him, to the same considerations; and it is to Mr. Joule that we are indebted for those important and laborious experimental researches in that department where the applicability of the law of the conservation of force appeared most doubtful, and where the greatest gaps in actual knowledge occurred, namely, in the production of work from heat, and of heat from work.

To state the law clearly it was necessary, in contradistinction to Galileo's conception of the *intensity of force*, that a new mechanical idea was elaborated, which we may term the conception of the *quantity of force*, and which has also been called *quantity of work* or of *energy*.

A way to this conception of the quantity of force had been prepared partly, in theoretical mechanics, through the conception of the *amount of vis viva* of a moving body, and partly by practical mechanics through the conception of the *motive power* necessary to keep a machine at work. Practical machinists had already found a standard by which any motive power could be measured, in the determination of the number of pounds that it could lift one foot in a second; and, as is known, a horse-power was defined to be equivalent to the motive power required to lift seventy kilogrammes one metre in each second.

Machines, and the motive powers required for their movement, furnish, in fact, the most familiar illustrations of the uniformity of all natural forces expressed by the law of the conservation of force. Any machine which is to be set in motion requires a mechanical motive power. Whence this power is derived or what its form, is of no consequence, provided only it be sufficiently great and act continuously. At one time we employ a steam-engine, at another a water-wheel or turbine, here horses or oxen at a whim, there a windmill, or if but little power is required, the human arm, a raised weight, or an electro-magnetic engine. The choice of the machine is merely dependent on the amount of power we would use, or the force of circumstance. In the watermill the weight of the water flowing down the hills is the agent; it is lifted to the hills by a meteorological process, and becomes the source of motive power for the mill. In the windmill it is the *vis viva* of the moving air which drives round the sails; this motion also is due to a meteorological operation of the atmosphere. In the steam-engine we have the tension of the heated vapour which drives the piston to and fro; this is engendered by the heat arising from the combustion of the coal in the fire-box, in other words, by a chemical process; and in this case the latter action is the source of the motive power. If it be a horse or the human arm which is at work, we have the muscles stimulated through the nerves, directly producing the mechanical force. In order, however, that the living body may generate muscular power it must be nourished and breathe. The food it takes separates again from it, after having combined with the oxygen inhaled from the air, to form carbonic acid and water. Here again, then, a chemical process is an essential element to maintain muscular power. A similar state of things is observed in the electro-magnetic machines of our telegraphs.

Thus, then, we obtain mechanical motive force from the most varied processes of nature in the most different ways; but it will also be remarked in only a limited quantity. In doing so we always *consume* something that nature supplies to us. In the watermill we use a quantity of water collected at an elevation, coal in the steam-engine, zinc and sulphuric acid in the electro-magnetic machine, food for the horse; in the windmill we use up the motion of the wind, which is arrested by the sails.

Conversely, if we have a motive force at our disposal we can develop with it forms of action of the most varied kind. It will not be necessary in this place to enumerate the countless diversity of industrial machines, and the varieties of work which they perform.

Let us rather consider the physical differences of the possible performance of a motive power. With its help we can raise loads, pump water to an elevation, compress gases, set a railway train in motion, and through friction generate heat. By its aid we can turn magneto-electric machines, and produce electric currents, and with them decompose water and other chemical compounds having the most powerful affinities, render wires incandescent, magnetise iron, &c.

Moreover, had we at our disposal a sufficient mechanical motive force we could restore all those states and conditions from which, as was seen above, we are enabled at the outset to derive mechanical motive power.

As, however, the motive power derived from any given natural process is limited, so likewise is there a limitation to the total amount of modifications which we may produce by the use of any given motive power.

These deductions, arrived at first in isolated instances from machines and physical apparatus, have now been welded into a law of nature of the widest validity. Every change in nature is equivalent to a certain development,

or a certain consumption of motive force. If motive power be developed it may either appear as such, or be directly used up again to form other changes equivalent in magnitude. The leading determinations of this equivalency are founded on Joule's measurements of the mechanical equivalent of heat. When, by the application of heat, we set a steam-engine in motion, heat proportional to the work done disappears within it; in short, the heat which can warm a given weight of water one degree of the Centigrade scale is able, if converted into work, to lift the same weight of water to a height of 425 metres. If we convert work into heat by friction we again use, in heating a given weight of water one degree Centigrade, the motive force which the same quantity of water would have generated in flowing down from a height of 425 metres. Chemical processes generate heat in definite proportion, and in like manner we estimate the motive power equivalent to such chemical forces; and thus the energy of the chemical force of affinity is also measurable by the mechanical standard. The same holds true for all the other forms of natural forces, but it will not be necessary to pursue the subject further here.

It has actually been established, then, as a result of these investigations, that all the forces of nature are measurable by the same mechanical standard, and that all pure motive forces are, as regards performance of work, equivalent. And thus one great step towards the solution of the comprehensive theoretical task of referring all natural phenomena to motion has been accomplished.

Whilst the foregoing considerations chiefly seek to elucidate the logical value of the law of the conservation of force, its actual signification in the general conception of the processes of nature is expressed in the grand connection which it establishes between the entire proce^ses

of the universe, through all distances of place or time. The universe appears, according to this law, to be endowed with a store of energy which, through all the varied changes in natural processes, can neither be increased nor diminished, which is maintained therein in ever-varying phases, but, like matter itself, is from eternity to eternity of unchanging magnitude; *acting in space*, but not *divisible*, as matter is, with it. Every change in the world simply consists in a variation in the mode of appearance of this store of energy. Here we find one portion of it as the *vis viva* of moving bodies, there as regular oscillation in light and sound; or, again, as heat, that is to say, the irregular motion of invisible particles; at another point the energy appears in the form of the weight of two masses gravitating towards each other, then as internal tension and pressure of elastic bodies, or as chemical attraction, electrical tension, or magnetic distribution. If it disappear in one form, it reappears as surely in another; and whenever it presents itself in a new phase we are certain that it does so at the expense of one of its other forms.

Carnot's law of the mechanical theory of heat, as modified by Clausius, has, in fact, made it clear that this change moves in the main continuously onward in a definite direction, so that a constantly increasing amount of the great store of energy in the universe is being transformed into heat.

We can, therefore, see with the mind's eye the original condition of things in which the matter composing the celestial bodies was still cold, and probably distributed as chaotic vapour or dust through space; we see that it must have developed heat when it collected together under the influence of gravity. Even at the present time spectrum analysis (a method the theoretical principles of which owe their origin to the mechanical theory

of heat) enables us to detect remains of this loosely distributed matter in the nebulæ; we recognise it in the meteor-showers and comets; the act of agglomeration and the development of heat still continue, though in our portion of the stellar system they have ceased to a great extent. The chief part of the primordial energy of the matter belonging to our system is now in the form of solar heat. This energy, however, will not remain locked up in our system for ever: portions of it are continually radiating from it, in the form of light and heat, into infinite space. Of this radiation our earth receives a share. It is these solar heat-rays which produce on the earth's surface the winds and the currents of the ocean, and lift the watery vapour from the tropical seas, which, distilling over hill and plain, returns as springs and rivers to the sea. The solar rays impart to the plant the power to separate from carbonic acid and water those combustible substances which serve as food for animals, and thus, in even the varied changes of organic life, the moving power is derived from the infinitely vast store of the universe.

This exalted picture of the connection existing between all the processes of nature has been often presented to us in recent times; it will suffice here that I direct attention to its leading features. If the task of physical science be to determine laws, a step of the most comprehensive significance towards that object has here been taken.

The application of the law of the conservation of force to the vital processes of animals and plants, which has just been discussed, leads us in another direction in which our knowledge of nature's conformity to law has made an advance. The law to which we referred is of the most essential importance in leading questions of physiology, and it was for this reason that Dr. Mayer

and I were led on physiological grounds to investigations having especial reference to the conservation of force.

As regards the phenomena of inorganic nature all doubts have long since been laid to rest respecting the principles of the method. It was apparent that these phenomena had fixed laws, and examples enough were already known to make the finding of such laws probable.

In consequence, however, of the greater complexity of the vital processes, their connection with mental action, and the unmistakable evidence of adaptability to a purpose which organic structures exhibit, the existence of a settled conformity to law might well appear doubtful, and, in fact, physiology has always had to encounter this fundamental question: are all vital processes absolutely conformable to law? Or is there, perhaps, a range of greater or less magnitude within which an exception prevails? More or less obscured by words, the view of Paracelsus, Helmont, and Stahl, has been, and is at present, held, particularly outside Germany, that there exists a soul of life ("*Lebensseele*") directing the organic processes which is endowed more or less with consciousness like the soul of man. The influence of the inorganic forces of nature on the organism was still recognised on the assumption that the soul of life only exercises power over matter by means of the physical and chemical forces of matter itself; so that without this aid it could accomplish nothing, but that it possessed the faculty of suspending or permitting the operation of the forces at pleasure.

After death, when no longer subject to the control of the soul of life or vital force, it was these very chemical forces of organic matter which brought about decomposition. In short, through all the different modes of expressing it, whether it was termed the Archäus, the *anima inscia*, or the *vital force* and the *restorative*

power of nature, the faculty to build up the body according to system, and to suitably accommodate it to external circumstances, remained the most essential attribute of this hypothetically controlling principle of the vitalistic theory with which, therefore, by reason of its attributes, only the name of soul fully harmonised.

It is apparent, however, that this notion runs directly counter to the law of the conservation of force. If vital force were for a time to annul the gravity of a weight, it could be raised without labour to any desired height, and subsequently, if the action of gravity were again restored, could perform work of any desired magnitude. And thus work could be obtained out of nothing without expense. If vital force could for a time suspend the chemical affinity of carbon for oxygen, carbonic acid could be decomposed without work being employed for that purpose, and the liberated carbon and oxygen could perform new work.

In reality, however, no trace of such an action is to be met with as that of the living organism being able to generate an amount of work without an equivalent expenditure. When we consider the work done by animals, we find the operation comparable in every respect with that of the steam-engine. Animals, like machines, can only move and accomplish work by being continuously supplied with fuel (that is to say, food) and air containing oxygen; both give off again this material in a burnt state, and at the same time produce heat and work. All investigation, thus far, respecting the amount of heat which an animal produces when at rest is in no way at variance with the assumption that this heat exactly corresponds to the equivalent, expressed as work, of the forces of chemical affinity then in action.

As regards the work done by plants, a source of power in every way sufficient, exists in the solar rays which they

require for the increase of the organic matter of their structures. Meanwhile it is true that exact quantitative determinations of the equivalents of force, consumed and produced in the vegetable as well as the animal kingdom, have still to be made in order to fully establish the exact accordance of these two values.

If, then, the law of the conservation of force hold good also for the living body, it follows that the physical and chemical forces of the material employed in building up the body are in continuous action without intermission and without choice, and that *their exact conformity to law never suffers a moment's interruption.*

Physiologists, then, must expect to meet with an unconditional conformity to law of the forces of nature in their inquiries respecting the vital processes; they will have to apply themselves to the investigation of the physical and chemical processes going on within the organism. It is a task of vast complexity and extent; but the workers, in Germany especially, are both numerous and enthusiastic, and we may already affirm that their labours have not been unrewarded, inasmuch as our knowledge of the vital phenomena has made greater progress during the last forty years than in the two preceding centuries.

Assistance, that cannot be too highly valued, towards the elucidation of the fundamental principles of the doctrine of life, has been rendered on the part of descriptive natural history, through Darwin's theory of the evolution of organic forms, by which the possibility of an entirely new interpretation of organic adaptability is furnished.

The adaptability in the construction of the functions of the living body, most wonderful at any time, and with the progress of science becoming still more so, was doubtless the chief reason that provoked a comparison

of the vital processes with the actions of a principle acting like a soul. In the whole external world we know of but one series of phenomena possessing similar characteristics, we mean the actions and deeds of an intelligent human being; and we must allow that in innumerable instances the organic adaptability appears to be so extraordinarily superior to the capacities of the human intelligence that we might feel disposed to ascribe to it a higher rather than a lower character.

Before the time of Darwin only two theories respecting organic adaptability were in vogue, both of which pointed to the interference of free intelligence in the course of natural processes. On the one hand it was held, in accordance with the vitalistic theory, that the vital processes were continuously directed by a living soul; or, on the other, recourse was had to an act of supernatural intelligence to account for the origin of every living species. The latter view indeed supposes that the causal connection of natural phenomena had been broken less often, and allows of a strict scientific examination of the processes observable in the species of human beings now existing; but even it is not able to entirely explain away those exceptions to the law of causality, and consequently it enjoyed no considerable favour as opposed to the vitalistic view, which was powerfully supported, by apparent evidence, that is, by the natural desire to find similar causes behind similar phenomena.

Darwin's theory contains an essentially new creative thought. It shows how adaptability of structure in organisms can result from a blind rule of a law of nature without any intervention of intelligence. I allude to the law of transmission of individual peculiarities from parent to offspring, a law long known and recognised, and only needing a more precise definition. If both parents have individual peculiarities

in common, the majority of their offspring also possess them; and if among the offspring there are some which present these peculiarities in a less marked degree, there will, on the other hand, always be found among a great number, others in which the same peculiarities have become intensified. If, now, these be selected to propagate offspring, a greater and greater intensification of these peculiarities may be attained and transmitted. This is, in fact, the method employed in cattle-breeding and gardening, in order with greater certainty to obtain new breeds and varieties, with well-marked different characters. The experience of artificial breeding is to be regarded, from a scientific point of view, as an experimental confirmation of the law under discussion; and, in fact, this experiment has proved successful, and is still doing so, with species of every class of the animal kingdom, and, with respect to the most different organs of the body, in a vast number of instances.

After the general application of the law of transmission had been established in this way, it only remained for Darwin to discuss the bearings of the question as regards animals and plants in the wild state. The result which has been arrived at is that those individuals which are distinguished in the struggle for existence by some advantageous quality, are the most likely to produce offspring, and thus transmit to them their advantageous qualities. And in this way from generation to generation a gradual adjustment is arrived at in the adaptation of each species of living creation to the conditions under which it has to live until the type has reached such a degree of perfection that any substantial variation from it is a disadvantage. It will then remain unchanged so long as the external conditions of its existence remain materially unaltered. Such an almost absolutely fixed condition appears to

be attained by the plants and animals now living, and thus the continuity of the species, at least during historic times, is found to prevail.

An animated controversy, however, still continues, concerning the truth or probability of the Darwinian theory, for the most part respecting the limits that should be assigned to the variation of species. The opponents of this view would hardly deny that, as assumed by Darwin, hereditary differences of race could have arisen in one and the same species; or, in other words, that many of the forms hitherto regarded as distinct species of the same genus have been derived from the same primitive form. Whether we must restrict our view to this, or whether, perhaps, we venture to derive all mammals from one original marsupial, or, again, all vertebrates from a primitive lancelet, or all plants and animals together from the slimy protoplasm of a protiston, depends at the present moment rather on the leanings of individual observers than on facts. Fresh links, connecting classes of apparently irreconcilable type, are always presenting themselves; the actual transition of forms, into others widely different, has already been traced in regularly deposited geological strata, and has come to be beyond question; and since this line of research has been taken up, how numerous are the facts which fully accord with Darwin's theory, and give special effect to it in detail!

At the same time, we should not forget the clear interpretation Darwin's grand conception has supplied of the till then mysterious notions respecting natural affinity, natural systems, and homology of organs in various animals; how by its aid the remarkable recurrence of the structural peculiarities of lower animals in the embryos of others higher in the scale, the special kind of development appearing in the series of palæontological forms, and the peculiar conditions of affinity of the faunas

and floras of limited areas have, one and all, received elucidation. Formerly natural affinity appeared to be a mere enigmatical, and altogether groundless similarity of forms; now it has become a matter for actual consanguinity. The natural system certainly forced itself as such upon the mind, although theory strictly disavowed any real significance to it; at present it denotes an actual genealogy of organisms. The facts of palæontological and embryological evolution and of geographical distribution were enigmatical wonders so long as each species was regarded as the result of an independent act of creation, and cast a scarcely favourable light on the strange tentative method which was ascribed to the Creator. Darwin has raised all these isolated questions from the condition of a heap of enigmatical wonders to a great consistent system of development, and established definite ideas in the place of such a fanciful hypothesis as, among the first, had occurred to Goethe, respecting the facts of the comparative anatomy and the morphology of plants.

This renders possible a definite statement of problems for further inquiry, a great gain in any case, even should it happen that Darwin's theory does not embrace the whole truth, and that, in addition to the influences which he has indicated, there should be found to be others which operate in the modification of organic forms.

While the Darwinian theory treats exclusively of the gradual modification of species after a succession of generations, we know that a single individual may adapt itself, or become accustomed, in a certain degree, to the circumstances under which it has to live; and that even during the single life of an individual a distinct progress towards a higher development of organic adaptability may be attained. And it is more especially in those forms of organic life where the adaptability in structure

has reached the highest grade and excited the greatest admiration, namely, in the region of mental perception, that, as the latest results of physiology teach us, this individual adaptation plays a most prominent part.

Who has not marvelled at the fidelity and accuracy of the information which our senses convey to us from the surrounding world, more especially those of the far-reaching eye? The information so gained furnishes the premisses for the conclusions which we come to, the acts that we perform; and unless our senses convey to us correct impressions, we cannot expect to act accurately, so that results shall correspond with our expectations. By the success or failure of our acts we again and again test the truth of the information with which our senses supply us, and experience, after millions of repetitions, shows us that this fidelity is exceedingly great, in fact, almost free from exceptions. At all events, these exceptions, the so-called illusions of the senses, are rare, and are only brought about by very special and unusual circumstances.

Whenever we stretch forth the hand to lay hold of something, or advance the foot to step upon some object, we must first form an accurate optical image of the position of the object to be touched, its form, distance, &c., or we shall fail. The certainty and accuracy of our perception by the senses must at least equal the certainty and accuracy which our actions have attained after long practice; and the belief, therefore, in the trustworthiness of our senses is no blind belief, but one, the accuracy of which has been tested and verified again and again by numberless experiments.

Were this harmony between the perceptions through the senses and the objects causing them, in other words, this basis of all our knowledge, a direct product of the vital principle, its formative power would, in fact, then have attained the highest degree of perfection. But an

examination of the actual facts at once destroys in the most merciless manner all belief in a preordained harmony of the inner and external world.

I need not call to mind the startling and unexpected results of ophthalmometrical and optical research which have proved the eye to be a by no means more perfect optical instrument than those constructed by human hands; but, on the contrary, to exhibit, in addition to the faults inseparable from any dioptric instrument, others that in an artificial instrument we should severely condemn; nor need I remind you that the ear conveys to us sounds from without in no wise in the ratio of their actual intensity, but strangely resolves them and modifies them, intensifying or weakening them in very different degrees, according to their varieties of pitch.

These anomalies, however, are as nothing compared with those to be met with in examining the nature of the sensations by which we become acquainted with the various properties of the objects surrounding us. Here it can at once be proved that no kind and no degree of similarity exists between the quality of a sensation and the quality of the agent inducing it, and portrayed by it.

In its leading features this was demonstrated by Johannes Müller in his law of the Specific Action of the Senses. According to him, each nerve of sense possesses a peculiar kind of sensation. A nerve, we know, can be rendered active by a vast number of exciting agents, and the same agent may likewise affect different organs of sense; but however it be brought about, we never have in nerves of sight any other sensation than that of light; in the nerves of the ear any other than a sensation of sound; in short, in each individual nerve of sense only that sensation which corresponds to its peculiar specific action. The most marked differences in the qualities of sensation, in other words, those between the sensations of different

senses, are, then, in no way dependent on the nature of the exciting agent, but only on that of the nerve apparatus under operation.

The bearing of Müller's law has been extended by later research. It appears highly probable that even the sensations of different colours and different pitch, as well as qualitative peculiarities of luminous sensations *inter se*, and of sonorous sensations *inter se*, also depend on the excitation of systems of fibres, with distinct character and endowed with different specific energy, of nerves of sight and hearing respectively. The infinitely more varied diversity of composite light is in this way referable to sensations of only threefold heterogeneous character, in other words, to mixtures of the three primary colours. From this reduction in the number of possible differences it follows that very different composite light may appear the same. In this case it has been shown that no kind of physical similarity whatever corresponds to the subjective similarity of different composite light of the same colour. By these and similar facts we are led to the very important conclusion that our sensations are, as regards their quality, only *signs* of external objects, and in no sense *images* of any degree of resemblance. An image must, in certain respects, be *analogous* to the original object; a statue, for instance, has the same corporeal form as the human being after which it is made; a picture the same colour and perspective projection. For a *sign* it is sufficient that it become apparent as often as the occurrence to be depicted makes its appearance, the conformity between them being restricted to their presenting themselves simultaneously; and the correspondence existing between our sensations and the objects producing them is precisely of this kind. They are signs which we have *learned to decipher*, and a language given us with our

organization by which external objects discourse to us—a language, however, like our mother tongue, that we can only learn by practice and experience.

Moreover, what has been said holds good not only for the qualitative differences of sensations, but also, in any case, for the greatest and most important part, if not the whole, of our various perceptions of extension in space. In their bearings on this question the new doctrine of binocular vision and the invention of the stereoscope have been of importance. All that the sensation of the two eyes could convey to us directly, and without psychical aid was, at the most, two somewhat different flat pictures of two dimensions as they lay on the two retinæ; instead of this we perceive a representation with three dimensions of the things around us. We are sensible as well of the *distance* of objects not too far removed from us as of their perspective *juxtaposition*, and compare the actual magnitude of two objects of apparently unequal size at different distances from us with greater certainty than the apparent equal magnitudes of a finger, say, and the moon.

One explanation only of our perception of extension in space, which stands the test of each separate fact, can in my judgment be brought forward by our assuming with Lotze that to the sensations of nerve-fibres, differently situated in space, certain differences, *local signs*, attach themselves, the significations of which, as regards space, we have to learn. That a knowledge of their signification may be attained by these hypotheses, and with the help of the movements of our body, and that we can at the same time learn which are the right movements to bring about a desired result, and become conscious of having arrived at it, has in many ways been established.

That experience exercised an enormous influence over

the signification of visual pictures, and, in cases of doubt, is generally the final arbiter, is allowed even by those physiologists who wish to save as much as possible of the innate harmony of the senses with the external world. The controversy is at present almost entirely confined to the question of the proportion at birth of the innate impulses that can facilitate training in the understanding of sensations. The assumption of the existence of impulses of this kind is unnecessary, and renders difficult instead of elucidating an interpretation of well-observed phenomena in adults.[1]

It follows, then, that this subtile and most admirable harmony existing between our sensations and the objects causing them is substantially, and with but few doubtful exceptions, a conformity individually acquired, a result of experience, of training, the recollection of former acts of a similar kind.

This completes the circle of our observations, and lands us at the spot whence we set out. We found at the beginning, that what physical science strives after is the knowledge of laws, in other words, the knowledge how at different times under the same conditions the same results are brought about; and we found in the last instance how all laws can be reduced to laws of motion. We now find, in conclusion, that our sensations are merely signs of changes taking place in the external world, and can only be regarded as pictures in that they represent succession in time. For this very reason they are in a position to show directly the *conformity to law*, in regard to succession in time, of natural phenomena. If, under the same natural circumstances, the same action take place, a person observing it under the same conditions will find the same series of impressions regularly recur.

[1] A further exposition of these conditions will be found in the lectures on the Recent Progress of the Theory of Vision, pp. 197 *et seq.*

That which our organs of sense perform is clearly sufficient to meet the demands of science as well as the practical ends of the man of business who must rely for support on the knowledge of natural laws, acquired, partly involuntarily by daily experience, and partly purposely by the study of science.

Having now completed our survey, we may, perhaps, strike a not unsatisfactory balance. Physical science has made active progress, not only in this or that direction, but as a vast whole, and what has been accomplished may warrant the attainment of further progress. Doubts respecting the entire conformity to law of nature are more and more dispelled; laws more general and more comprehensive have revealed themselves. That the direction which scientific study has taken is a healthy one its great practical issues have clearly demonstrated; and I may here be permitted to direct particular attention to the branch of science more especially my own. In physiology particularly scientific work had been crippled by doubts respecting the necessary conformity to law, which means, as we have shown, the intelligibility of vital phenomena, and this naturally extended itself to the practical science directly dependent on physiology, namely, medicine. Both have received an impetus, such as had not been felt for thousands of years, from the time that they seriously adopted the method of physical science, the exact observation of phenomena and experiment. As a practising physician, in my earlier days, I can personally bear testimony to this. I was educated at a period when medicine was in a transitional stage, when the minds of the most thoughtful and exact were filled with despair. It was not difficult to recognise that the old predominant theorising methods of practising medicine were altogether untenable; with these theories, however, the facts on which they had actually been founded had

become so inextricably entangled that they also were mostly thrown overboard. How a science should be built up anew had already been seen in the case of the other sciences; but the new task assumed colossal proportions; few steps had been taken towards accomplishing it, and these first efforts were in some measure but crude and clumsy. We need feel no astonishment that many sincere and earnest men should at that time have abandoned medicine as unsatisfactory, or on principle given themselves over to an exaggerated empiricism.

But well directed efforts produced the right result more quickly even than many had hoped for. The application of the mechanical ideas to the doctrine of circulation and respiration, the better interpretation of thermal phenomena, the more refined physiological study of the nerves, soon led to practical results of the greatest importance; microscopic examination of parasitic structures, the stupendous development of pathological anatomy, irresistibly led from nebulous theories to reality. We found that we now possessed a much clearer means of distinguishing, and a clearer insight into the mechanism of the process of disease than the beats of the pulse, the urinary deposit, or the fever type of older medical science had ever given us. If I might name one department of medicine in which the influence of the scientific method has been, perhaps, most brilliantly displayed, it would be in ophthalmic medicine. The peculiar constitution of the eye enables us to apply physical modes of investigation as well in functional as in anatomical derangements of the living organ. Simple physical expedients, spectacles, sometimes spherical, sometimes cylindrical or prismatic, suffice, in many cases, to cure disorders which in earlier times left the organ in a condition of chronic incapacity; a great number of changes on the other hand, which formerly did not attract notice

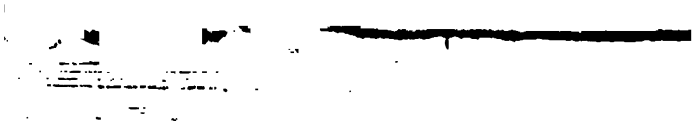
till they induced incurable blindness, can now be detected and remedied at the outset. From the very reason of its presenting the most favourable ground for the application of the scientific method, ophthalmology has proved attractive to a peculiarly large number of excellent investigators, and rapidly attained its present position, in which it sets an example to the other departments of medicine, of the actual capabilities of the true method, as brilliant as that which astronomy for long had offered to the other branches of physical science.

Though in the investigation of inorganic nature the several European nations showed a nearly uniform advancement, the recent progress of physiology and medicine is pre-eminently due to Germany. I have already spoken of the obstacles which formerly delayed progress in this direction. Questions respecting the nature of life are closely bound up with psychological and ethical inquiries. It demands, moreover, that we bestow on it unwearied diligence for purely ideal purposes, without any approaching prospect of the pure science becoming of practical value. And we may make it our boast that this exalted and self-denying assiduity, this labour for inward satisfaction, not for external success, has at all times peculiarly distinguished the scientific men of Germany.

What has, after all, determined the state of things in the present instance is in my opinion another circumstance, namely, that we are more fearless than others of the consequences of the entire and perfect truth. Both in England and France we find excellent investigators who are capable of working with thorough energy in the proper sense of the scientific methods; hitherto, however, they have almost always had to bend to social or ecclesiastical prejudices, and could only openly express their convictions at the expense of their social influence and their usefulness.

Germany has advanced with bolder step: she has had the full confidence, which has never been shaken, that truth fully known brings with it its own remedy for the danger and disadvantage that may here and there attend a limited recognition of what is true. A labour-loving, frugal, and moral people may exercise such boldness, may stand face to face with truth; it has nothing to fear though hasty or partial theories be advocated, even if they should appear to trench upon the foundations of morality and society.

We have met here on the southern frontier of our country. In science, however, we recognise no political boundaries, for our country reaches as far as the German tongue is heard, wherever German industry and German intrepidity in striving after truth find favour. And that it finds favour here is shown by our hospitable reception, and the inspiriting words with which we have been greeted. A new medical faculty has been established here. We will wish it in its career rapid progress in the cardinal virtues of German science, for then it will not only find remedies for bodily suffering, but become an active centre to strengthen intellectual independence, steadfastness to conviction and love of truth, and at the same time be the means of deepening the sense of unity throughout our country.



HYPNOTISM, MESMERISM, AND THE NEW WITCHCRAFT. By ERNEST HART, formerly Surgeon to the West London Hospital, and Ophthalmic Surgeon to St. Mary's Hospital, London. New edition, enlarged and revised. With new chapters on "The Eternal Gullible" and "The Hypnotism of Trilby." 12mo. Cloth, $1.50.

"Mr. Hart holds it as proved beyond all reasonable doubt that the hypnotic condition is an admitted clinical fact, and declares that the practice of hypnotism, except by skilled physicians, should be forbidden. He affirms its therapeutic uselessness, and condemns the practice because of the possibilities of social mischiefs. . . . His personal experiences in the 'New Witchcraft' enable him to exercise a critical check on the wild theories and unsupported assertions of others."—*Philadelphia Ledger.*

MESMERISM, SPIRITUALISM, ETC., HISTORICALLY AND SCIENTIFICALLY CONSIDERED. By WILLIAM B. CARPENTER, M. D., F. R. S. 12mo. Cloth, $1.25.

"The reader of these lectures will see that my whole aim is to discover, on the generally accepted principles of testimony, what *are* facts; and to discriminate between facts and the inferences drawn from them. I have no other 'theory' to support than that of the constancy of the well-ascertained laws of Nature."—*From the Preface.*

PRINCIPLES OF MENTAL PHYSIOLOGY. With their Application to the Training and Discipline of the Mind, and the Study of its Morbid Conditions. By WILLIAM B. CARPENTER, M. D., F. R. S. 12mo. Cloth, $3.00.

"Among the numerous eminent writers this country has produced none are more deserving of praise for having attempted to apply the results of physiological research to the explanation of the mutual relations of the mind and body than Dr. Carpenter."—*London Lancet.*

NATURE AND MAN: Essays, Scientific and Philosophical. By WILLIAM B. CARPENTER, M. D., F. R. S. With an Introductory Memoir by J. ESTLIN CARPENTER, M. A., and a Portrait. 12mo. Cloth, $2.25.

"Few works could be mentioned that give a better general view of the change that has been wrought in men's conceptions of life and Nature. For this, if for nothing else, the collection would be valuable. But it will be welcomed also as a kind of biography of its author, for the essays and the memoir support one another and are mutually illuminative."—*Scotsman.*

"Mr. Estlin Carpenter's memoir of his father is just what such a memoir should be—a simple record of a life uneventful in itself, whose interest for us lies mainly in the nature of the intellectual task so early undertaken, so strenuously carried on, so ample and nobly accomplished, to which it was devoted."—*London Spectator.*

D. APPLETON & CO.'S PUBLICATIONS.

FAMILIAR FLOWERS OF FIELD AND GARDEN. By F. SCHUYLER MATHEWS. Illustrated with 200 Drawings by the Author, and containing an elaborate Index showing at a glance the botanical and popular names, family, color, locality, environment, and time of bloom of several hundred flowers. 12mo. Library Edition, cloth, $1.75; Pocket Edition, flexible covers, $2.25.

In this convenient and useful volume the flowers which one finds in the fields are identified, illustrated, and described in familiar language. Their connection with garden flowers is made clear. Particular attention is drawn to the beautiful ones which have come under cultivation, and, as the title indicates, the book furnishes a ready guide to a knowledge of wild and cultivated flowers alike.

"I have examined Mr. Mathews's little book upon 'Familiar Flowers of Field and Garden,' and I have pleasure in commending the accuracy and beauty of the drawings and the freshness of the text. We have long needed some botany from the hand of an artist, who sees form and color without the formality of the scientist. The book deserves a reputation."—*L. H. Bailey, Professor of Horticulture, Cornell University.*

"I am much pleased with your 'Familiar Flowers of Field and Garden.' It is a useful and handsomely prepared handbook, and the elaborate index is an especially valuable part of it. Taken in connection with the many careful drawings, it would seem as though your little volume thoroughly covers its subject."—*Louis Prang.*

"The author describes in a most interesting and charming manner many familiar wild and cultivated plants, enlivening his remarks by crisp epigrams, and rendering identification of the subjects described simple by means of some two hundred drawings from Nature, made by his own pen. . . . The book will do much to more fully acquaint the reader with those plants of field and garden treated upon with which he may be but partly familiar, and go a long way toward correcting many popular errors existing in the matter of colors of their flowers, a subject to which Mr. Mathews has devoted much attention, and on which he is now a recognized authority in the trade."—*New York Florists' Exchange.*

"A book of much value and interest, admirably arranged for the student and the lover of flowers. . . . The text is full of compact information, well selected and interestingly presented. . . . It seems to us to be a most attractive handbook of its kind."—*New York Sun.*

"A delightful book and very useful. Its language is plain and familiar, and the illustrations are dainty works of art. It is just the book for those who want to be familiar with the well-known flowers, those that grow in the cultivated gardens as well as those that blossom in the fields."—*Newark Daily Advertiser.*

"Seasonable and valuable. The young botanist and the lover of flowers, who have only studied from Nature, will be greatly aided by this work."—*Pittsburg Post.*

"Charmingly written, and to any one who loves the flowers—and who does not?—will prove no less fascinating than instructive. It will open up in the garden and the fields a new world full of curiosity and delight, and invest them with a new interest in his sight."—*Christian Work.*

"One need not be deeply read in floral lore to be interested in what Mr. Mathews has written, and the more proficient one is therein the greater his satisfaction is likely to be."—*New York Mail and Express.*

"Mr. F. Schuyler Mathews's careful description and graceful drawings of our 'Familiar Flowers of Field and Garden' are fitted to make them familiar even to those who have not before made their acquaintance."—*New York Evening Post.*

New York: D. APPLETON & CO., 72 Fifth Avenue.

FAMILIAR TREES AND THEIR LEAVES.

By F. SCHUYLER MATHEWS, author of "Familiar Flowers of Field and Garden," "The Beautiful Flower Garden," etc. Illustrated with over 200 Drawings from Nature by the Author. 12mo. Cloth, $1.75.

"It is not often that we find a book which deserves such unreserved commendation. It is commendable for several reasons: it is a book that has been needed for a long time, it is written in a popular and attractive style, it is accurately and profusely illustrated, and it is by an authority on the subject of which it treats."—*Public Opinion.*

"Most readers of the book will find a world of information they never dreamed of about leaves that have long been familiar with them. The study will open to them new sources of pleasure in every tree around their houses, and prove interesting as well as instructive."—*San Francisco Call.*

"A revelation of the sweets and joys of natural things that we are too apt to pass by with but little or no thought. The book is somewhat more than an ordinary botanical treatise on leaves and trees. It is a heart-to heart talk with Nature, a true appreciation of the beauty and the real usefulness of leaves and trees."—*Boston Courier.*

"Has about it a simplicity and a directness of purpose that appeal at once to every lover of Nature."—*New York Mail and Express.*

"Mr. Mathews's book is just what is needed to open our eyes. His text is charming, and displays a loving and intimate acquaintance with tree life, while the drawings of foliage are beautifully executed. We commend the volume as a welcome companion in country walks."—*Philadelphia Public Ledger.*

"The book is one to read, and then to keep at hand for continual reference."—*Chicago Dial.*

"The unscientific lover of Nature will find this book a source of enjoyment as well as of instruction, and it will be a valuable introduction to the more scientific study of the subject."—*Cleveland Plain Dealer.*

"This book will be found most satisfactory. It is a book which is needed, written by one who knows trees as he knows people."—*Minneapolis Journal.*

"A book of large value to the student. The reader gathers a wide and valuable knowledge which will awaken new interest in every tramp through the forest."—*Chicago Inter-Ocean.*

"A most admirable volume in many ways. It meets a distinct and widely felt want; the work is excellently done; its appearance is very timely. . . . Written in a clear and simple style, and requires no previous technical knowledge of botany to understand it."—*Baltimore News.*

"This very valuable book will be prized by all who love Nature."—*The Churchman.*

"Of the many Nature books that are constantly inviting the reader to leave pavement and wander in country bypaths, this one, with its scientific foundation, and its simplicity and clearness of style, is among the most alluring."—*St. Paul Pioneer-Press.*

THE GARDEN'S STORY; or, Pleasures and Trials of an Amateur Gardener. By GEORGE H. ELLWANGER. With Head and Tail Pieces by Rhead. 12mo. Cloth, extra, $1.50.

"Mr. Ellwanger's instinct rarely errs in matters of taste. He writes out of the fullness of experimental knowledge, but his knowledge differs from that of many a trained cultivator in that his skill in garden practice is guided by a refined æsthetic sensibility, and his appreciation of what is beautiful in nature is healthy, hearty, and catholic. His record of the garden year, as we have said, begins with the earliest violet, and it follows the season through until the witch-hazel is blossoming on the border of the wintry woods. . . . This little book can not fail to give pleasure to all who take a genuine interest in rural life."—*New York Tribune.*

THE ORIGIN OF CULTIVATED PLANTS. By ALPHONSE DE CANDOLLE. 12mo. Cloth, $2.00.

"Though a fact familiar to botanists, it is not generally known how great is the uncertainty as to the origin of many of the most important cultivated plants. . . . In endeavoring to unravel the matter, a knowledge of botany, of geography, of geology, of history, and of philosophy is required. By a combination of testimony derived from these sources M. de Candolle has been enabled to determine the botanical origin and geographical source of the large proportion of species he deals with."—*The Athenæum.*

THE FOLK-LORE OF PLANTS. By T. F. THISELTON DYER, M. A. 12mo. Cloth, $1.50.

"A handsome and deeply interesting volume. . . . In all respects the book is excellent. Its arrangement is simple and intelligible, its style bright and alluring. . . . To all who seek an introduction to one of the most attractive branches of folk-lore, this delightful volume may be warmly commended.—*Notes and Queries.*

FLOWERS AND THEIR PEDIGREES. By GRANT ALLEN, author of "Vignettes of Nature," etc. Illustrated. 12mo. Cloth, $1.50.

"No writer treats scientific subjects with so much ease and charm of style as Mr. Grant Allen. The study is a delightful one, and the book is fascinating to any one who has either love for flowers or curiosity about them."—*Hartford Courant.*

"Any one with even a smattering of botanical knowledge, and with either a heart or mind, must be charmed with this collection of essays."—*Chicago Evening Journal.*

THE GEOLOGICAL HISTORY OF PLANTS. By Sir J. WILLIAM DAWSON, F. R. S. Illustrated. 12mo. Cloth, $1.75.

"The object of this work is to give, in a connected form, a summary of the development of the vegetable kingdom in geological time. To the geologist and botanist the subject is one of importance with reference to their special pursuits, and one on which it has not been easy to find any convenient manual of information. It is hoped that its treatment in the present volume will also be found sufficiently simple and popular to be attractive to the general reader."—*From the Preface.*

New York: D. APPLETON & CO., 72 Fifth Avenue.

OUTINGS AT ODD TIMES. By CHARLES C. ABBOTT, author of "Days out of Doors" and "A Naturalist's Rambles about Home." 16mo. Cloth, gilt top, $1.25.

"A charming little volume, literally alone with Nature, for it discusses seasons and the fields, birds, etc., with the loving freedom of a naturalist born. Every page reads like a sylvan poem; and for the lovers of the beautiful in quiet out-door and out-of-town life, this beautifully bound and attractively printed little volume will prove a companion and friend."—*Rochester Union and Advertiser.*

A NATURALIST'S RAMBLES ABOUT HOME. By CHARLES C. ABBOTT. 12mo. Cloth, $1.50.

"The home about which Dr. Abbott rambles is clearly the haunt of fowl and fish, of animal and insect life; and it is of the habits and nature of these that he discourses pleasantly in this book. Summer and winter, morning and evening, he has been in the open air all the time on the alert for some new revelation of instinct, or feeling, or character on the part of his neighbor creatures. Most that he sees and hears he reports agreeably to us, as it was no doubt delightful to himself. Books like this, which are free from all the technicalities of science, but yet lack little that has scientific value, are well suited to the reading of the young. Their atmosphere is a healthy one for boys in particular to breathe."—*Boston Transcript.*

DAYS OUT OF DOORS. By CHARLES C. ABBOTT. 12mo. Cloth, $1.50.

"'Days out of Doors' is a series of sketches of animal life by Charles C. Abbott, a naturalist whose graceful writings have entertained and instructed the public before now. The essays and narratives in this book are grouped in twelve chapters, named after the months of the year. Under 'January' the author talks of squirrels, musk-rats, water-snakes, and the predatory animals that withstand the rigor of winter; under 'February' of frogs and herons, crows and blackbirds; under 'March' of gulls and fishes and foxy sparrows; and so on appropriately, instructively, and divertingly through the whole twelve."—*New York Sun.*

THE PLAYTIME NATURALIST. By Dr. J. E. TAYLOR, F. L. S., editor of "Science Gossip." With 366 Illustrations. 12mo. Cloth, $1.50.

"The work contains abundant evidence of the author's knowledge and enthusiasm, and any boy who may read it carefully is sure to find something to attract him. The style is clear and lively, and there are many good illustrations."—*Nature.*

THE ORIGIN OF FLORAL STRUCTURES through Insects and other Agencies. By the Rev. GEORGE HENSLOW, Professor of Botany, Queen's College. With numerous Illustrations. 12mo. Cloth, $1.75.

"Much has been written on the structure of flowers, and it might seem almost superfluous to attempt to say anything more on the subject, but it is only within the last few years that a new literature has sprung up, in which the authors have described their observations and given their interpretations of the uses of floral mechanisms, more especially in connection with the processes of fertilization."—*From Introduction.*

New York: D. APPLETON & CO., 72 Fifth Avenue.

CLIMBING IN THE HIMALAYAS. By WILLIAM MARTIN CONWAY, M. A., F. R. G. S., Vice-President of the Alpine Club, formerly Professor of Art in University College, Liverpool. With 200 Illustrations by A. D. MCCORMICK, and a Map. 8vo. Cloth, $10.00.

This work contains a minute record of one of the most important and thrilling geographical enterprises of the century—an expedition made in 1892, under the auspices of the Royal Geographical Society, the Royal Society, the British Association, and the Government of India. It included an examination of the glaciers at the head of the Bagrot Valley and the great passes in the neighborhood of Rakipushi (25,930 feet); an expedition to Hispar, at the foot of the longest glacier in the world outside the polar regions; the first definitely recorded passage of the Hispar Pass, the longest known pass in the world; and the ascent of Pioneer Peak (about 23,000 feet), the highest ascent yet authentically made. No better man could have been chosen for this important expedition than Mr. Conway, who has spent over twenty years in mountaineering work in the Alps. Already the author of nine published books, he has recorded his discoveries in this volume in the clear, incisive, and thrilling language of an expert.

"It would be hard to say too much in praise of this superb work. As a record of mountaineering it is almost, if not quite, unique. Among records of Himalayan exploration it certainly stands alone. . . . The farther Himalayas . . . have never been so faithfully—in other words, so poetically—presented as in the masterly delicate sketches with which Mr. McCormick has adorned this book."—*London Daily News.*

"This survey volume is a worthy record of a splendid journey. . . . The book is not merely the narrative of the best organized and most successful mountaineering expedition as yet made. It is a most valuable and minute account, based on first-hand evidence, of a most fascinating region of the heaven-soaring Himalayas."—*Pall Mall Gazette.*

"Mr. Conway's volume is a splendid record of a daring and adventurous scientific expedition. . . . As Mr. Whymper did for the Northern Andes, Mr. Conway has done for the Karakoram mountains."—*London Times.*

"It would be difficult to say which of the many classes of readers will welcome the work with the most appreciation in its fascinating pages. Mr. Conway's pen and Mr. McCormick's pencil have made their countrymen partners in their pleasure."—*London Standard.*

". . . In addition to this, Mr. Conway is a man of letters, a student (and a teacher, too) of art, a scholar in several languages: one, too, who knows the Latin names of plants, and the use of theodolite and plane table. From him, therefore, if from any one, the world had a right to expect a book that should combine accurate observation and interesting reporting with an original and acute record of impressions; nor will the world have any reason to be disappointed."—*London Athenæum.*

"With its three hundred illustrations we have seldom seen a volume which speaks to the eye and understanding so pleasantly and expressively on every page. . . . We have an exhaustive panorama of the Himalayan scenery, of the manner in which the rough marching was conducted, of ascents achieved under the most dangerous conditions, and of the troubles and humors of the shifting camps where the coolies rested from their labors."—*London Saturday Review.*

"Perhaps no book of recent date gives a simpler or at the same time more effective picture of the truly wonderful mountain regions lying behind the northern barrier of India than Mr. Conway's striking volume."—*London Telegraph.*

APPLETONS' HOME-READING BOOKS.

Edited by W. T. HARRIS, A. M., LL D.,

U. S. Commissioner of Education.

A comprehensive series of books presenting upon a symmetrical plan the best available literature in the various fields of human learning, selected with a view to the needs of students of all grades in supplementing their school studies and for home reading. It is believed that this project will fully solve the long-standing problem as to what kind of reading shall be furnished to the young, and what will most benefit them intellectually as well as morally.

NOW READY.

THE STORY OF THE BIRDS. By JAMES NEWTON BASKETT. 65 cents *net.*

"Mr. Baskett's book is not to be easily disposed of in a few words; it is out of the common run of popular ornithology, and decidedly original. We attest the author's competence for clear statement of facts, and the thorough readability of his whole book."—*The Nation.*

THE PLANT WORLD: Its Romances and Realities. Compiled and edited by FRANK VINCENT, M. A., author of "Actual Africa," etc. 60 cents *net.*

"Its interest will extend to every member of the family, to every one who loves Nature, for its information regarding the plant world will make the mysteries of springtime the more significant, the more beautiful."—*Boston Times.*

THE STORY OF OLIVER TWIST. By CHARLES DICKENS. Condensed for home and school reading by ELLA BOYCE KIRK. 60 cents *net.*

"The language is unchanged, expressions are not modified, but everything a child would be likely to skip has been elided. The action is thus accelerated to suit the most impatient reader."—*Chicago Evening Post.*

IN BROOK AND BAYOU; or, Life in the Still Waters. By CLARA KERN BAYLISS. 60 cents *net.*

In this volume the author introduces her readers to some of the interesting inhabitants of the microscopic world by the aid of numerous plates and full descriptive text. The account of the evolution of these minute creatures and their struggle for existence is given in a remarkably entertaining way, and makes the book as fascinating as a novel.

IN PRESS.

CURIOUS HOMES AND THEIR TENANTS. By JAMES CARTER BEARD.
CRUSOE'S ISLAND. By F. A. OBER.
UNCLE SAM'S SECRETS. By O. P. AUSTIN.
NATURAL HISTORY READERS, 5 vols. By J. F. TROEGER.
THE HALL OF SHELLS. By Mrs. A. S. HARDY.

(Others in preparation.)

D. APPLETON & CO.'S PUBLICATIONS.

THE LIBRARY OF USEFUL STORIES.

Each book complete in itself. By writers of authority in their various spheres. 16mo. Cloth, 40 cents per volume.

NOW READY.

THE STORY OF THE STARS. By G. F. CHAMBERS, F. R. A. S., author of "Handbook of Descriptive and Practical Astronomy," etc. With 24 Illustrations.

"The author presents his wonderful and at times bewildering facts in a bright and cheery spirit that makes the book doubly attractive."—*Boston Home Journal.*

THE STORY OF "PRIMITIVE" MAN. By EDWARD CLODD, author of "The Story of Creation," etc.

"No candid person will deny that Mr. Clodd has come as near as any one at this time is likely to come to an authentic exposition of all the information hitherto gained regarding the earlier stages in the evolution of mankind."—*New York Sun.*

THE STORY OF THE PLANTS. By GRANT ALLEN, author of "Flowers and their Pedigrees," etc.

"As fascinating in style as a first-class story of fiction, and is a simple and clear exposition of plant life."—*Boston Home Journal.*

THE STORY OF THE EARTH. By H. G. SEELEY, F. R. S., Professor of Geography in King's College, London. With Illustrations.

"It is doubtful if the fascinating story of the planet on which we live has been previously told so clearly and at the same time so comprehensively."—*Boston Advertiser.*

THE STORY OF THE SOLAR SYSTEM. By G. F. CHAMBERS, F. R. A. S.

"Any intelligent reader can get clear ideas of the movements of the worlds about us. . . . Will impart a wise knowledge of astronomical wonders."—*Chicago Inter-Ocean.*

THE STORY OF A PIECE OF COAL. By E. A. MARTIN, F. G. S.

"The value and importance of this volume are out of all proportion to its size and outward appearance."—*Chicago Record.*

THE STORY OF ELECTRICITY. By JOHN MUNRO, C. E.

"The book is an excellent one, crammed full of facts, and deserves a place not alone on the desk of the student, but on the workbench of the practical electrician."—*New York Times.*

THE STORY OF EXTINCT CIVILIZATIONS OF THE EAST. By ROBERT ANDERSON, M. A., F. A. S., author of "Early England," "The Stuart Period," etc.

New York · D. APPLETON & CO., 72 Fifth Avenue.

Stanford University Libraries
3 6105 020 001 447

STANFORD UNIVERSITY LIBRARIES
STANFORD AUXILIARY LIBRARY
STANFORD, CALIFORNIA 94305-6004
(650) 723-9201
salcirc@sulmail.stanford.edu
All books are subject to recall.
DATE DUE

MAR 2 2 2000
JUN 2 0 2000